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THE PSYCHOLOGICAL REVIEW

THE MAGICAL NUMBER SEVEN, PLUS OR MINUS TWO: SOME LIMITS ON OUR CAPACITY FOR PROCESSING INFORMATION[1]

GEORGE A. MILLER

Harvard University

My problem is that I have been persecuted by an integer. For seven years this number has followed me around, has intruded in my most private data, and has assaulted me from the pages of our most public journals. This number assumes a variety of disguises, being sometimes a little larger and sometimes a little smaller than usual, but never changing so much as to be unrecognizable. The persistence with which this number plagues me is far more than a random accident. There is, to quote a famous senator, a design behind it, some pattern governing its appearances. Either there really is something unusual about the number or else I am suffering from delusions of persecution.

I shall begin my case history by telling you about some experiments that tested how accurately people can assign numbers to the magnitudes of various aspects of a stimulus. In the traditional language of psychology these would be called experiments in absolute judgment. Historical accident, however, has decreed that they should have another name. We now call them experiments on the capacity of people to transmit information. Since these experiments would not have been done without the appearance of information theory on the psychological scene, and since the results are analyzed in terms of the concepts of information theory, I shall have to preface my discussion with a few remarks about this theory.

INFORMATION MEASUREMENT

The "amount of information" is exactly the same concept that we have talked about for years under the name of "variance." The equations are different, but if we hold tight to the idea that anything that increases the variance also increases the amount of information we cannot go far astray.

The advantages of this new way of talking about variance are simple enough. Variance is always stated in terms of the unit of measurement—inches, pounds, volts, etc.—whereas the amount of information is a dimensionless quantity. Since the information in a discrete statistical distribution does not depend upon the unit of measurement, we can extend the concept to situations where we have no metric and we would not ordinarily think of using

[1] This paper was first read as an Invited Address before the Eastern Psychological Association in Philadelphia on April 15, 1955. Preparation of the paper was supported by the Harvard Psycho-Acoustic Laboratory under Contract N5ori-76 between Harvard University and the Office of Naval Research, U. S. Navy (Project NR142-201, Report PNR-174). Reproduction for any purpose of the U. S. Government is permitted.

the variance. And it also enables us to compare results obtained in quite different experimental situations where it would be meaningless to compare variances based on different metrics. So there are some good reasons for adopting the newer concept.

The similarity of variance and amount of information might be explained this way: When we have a large variance, we are very ignorant about what is going to happen. If we are very ignorant, then when we make the observation it gives us a lot of information. On the other hand, if the variance is very small, we know in advance how our observation must come out, so we get little information from making the observation.

If you will now imagine a communication system, you will realize that there is a great deal of variability about what goes into the system and also a great deal of variability about what comes out. The input and the output can therefore be described in terms of their variance (or their information). If it is a good communication system, however, there must be some systematic relation between what goes in and what comes out. That is to say, the output will depend upon the input, or will be correlated with the input. If we measure this correlation, then we can say how much of the output variance is attributable to the input and how much is due to random fluctuations or "noise" introduced by the system during transmission. So we see that the measure of transmitted information is simply a measure of the input-output correlation.

There are two simple rules to follow. Whenever I refer to "amount of information," you will understand "variance." And whenever I refer to "amount of transmitted information," you will understand "covariance" or "correlation."

The situation can be described graphically by two partially overlapping circles. Then the left circle can be taken to represent the variance of the input, the right circle the variance of the output, and the overlap the covariance of input and output. I shall speak of the left circle as the amount of input information, the right circle as the amount of output information, and the overlap as the amount of transmitted information.

In the experiments on absolute judgment, the observer is considered to be a communication channel. Then the left circle would represent the amount of information in the stimuli, the right circle the amount of information in his responses, and the overlap the stimulus-response correlation as measured by the amount of transmitted information. The experimental problem is to increase the amount of input information and to measure the amount of transmitted information. If the observer's absolute judgments are quite accurate, then nearly all of the input information will be transmitted and will be recoverable from his responses. If he makes errors, then the transmitted information may be considerably less than the input. We expect that, as we increase the amount of input information, the observer will begin to make more and more errors; we can test the limits of accuracy of his absolute judgments. If the human observer is a reasonable kind of communication system, then when we increase the amount of input information the transmitted information will increase at first and will eventually level off at some asymptotic value. This asymptotic value we take to be the *channel capacity* of the observer: it represents the greatest amount of information that he can give us about the stimulus on the basis of an absolute judgment. The channel capacity is the upper limit on the extent to which the observer can match his responses to the stimuli we give him.

Now just a brief word about the *bit*

and we can begin to look at some data. One bit of information is the amount of information that we need to make a decision between two equally likely alternatives. If we must decide whether a man is less than six feet tall or more than six feet tall and if we know that the chances are 50–50, then we need one bit of information. Notice that this unit of information does not refer in any way to the unit of length that we use—feet, inches, centimeters, etc. However you measure the man's height, we still need just one bit of information.

Two bits of information enable us to decide among four equally likely alternatives. Three bits of information enable us to decide among eight equally likely alternatives. Four bits of information decide among 16 alternatives, five among 32, and so on. That is to say, if there are 32 equally likely alternatives, we must make five successive binary decisions, worth one bit each, before we know which alternative is correct. So the general rule is simple: every time the number of alternatives is increased by a factor of two, one bit of information is added.

There are two ways we might increase the amount of input information. We could increase the rate at which we give information to the observer, so that the amount of information per unit time would increase. Or we could ignore the time variable completely and increase the amount of input information by increasing the number of alternative stimuli. In the absolute judgment experiment we are interested in the second alternative. We give the observer as much time as he wants to make his response; we simply increase the number of alternative stimuli among which he must discriminate and look to see where confusions begin to occur. Confusions will appear near the point that we are calling his "channel capacity."

ABSOLUTE JUDGMENTS OF UNIDIMENSIONAL STIMULI

Now let us consider what happens when we make absolute judgments of tones. Pollack (17) asked listeners to identify tones by assigning numerals to them. The tones were different with respect to frequency, and covered the range from 100 to 8000 cps in equal logarithmic steps. A tone was sounded and the listener responded by giving a numeral. After the listener had made his response he was told the correct identification of the tone.

When only two or three tones were used the listeners never confused them. With four different tones confusions were quite rare, but with five or more tones confusions were frequent. With fourteen different tones the listeners made many mistakes.

These data are plotted in Fig. 1. Along the bottom is the amount of input information in bits per stimulus. As the number of alternative tones was increased from 2 to 14, the input information increased from 1 to 3.8 bits. On the ordinate is plotted the amount of

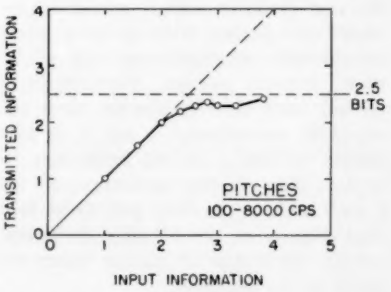

FIG. 1. Data from Pollack (17, 18) on the amount of information that is transmitted by listeners who make absolute judgments of auditory pitch. As the amount of input information is increased by increasing from 2 to 14 the number of different pitches to be judged, the amount of transmitted information approaches as its upper limit a channel capacity of about 2.5 bits per judgment.

transmitted information. The amount of transmitted information behaves in much the way we would expect a communication channel to behave; the transmitted information increases linearly up to about 2 bits and then bends off toward an asymptote at about 2.5 bits. This value, 2.5 bits, therefore, is what we are calling the channel capacity of the listener for absolute judgments of pitch.

So now we have the number 2.5 bits. What does it mean? First, note that 2.5 bits corresponds to about six equally likely alternatives. The result means that we cannot pick more than six different pitches that the listener will never confuse. Or, stated slightly differently, no matter how many alternative tones we ask him to judge, the best we can expect him to do is to assign them to about six different classes without error. Or, again, if we know that there were N alternative stimuli, then his judgment enables us to narrow down the particular stimulus to one out of $N/6$.

Most people are surprised that the number is as small as six. Of course, there is evidence that a musically sophisticated person with absolute pitch can identify accurately any one of 50 or 60 different pitches. Fortunately, I do not have time to discuss these remarkable exceptions. I say it is fortunate because I do not know how to explain their superior performance. So I shall stick to the more pedestrian fact that most of us can identify about one out of only five or six pitches before we begin to get confused.

It is interesting to consider that psychologists have been using seven-point rating scales for a long time, on the intuitive basis that trying to rate into finer categories does not really add much to the usefulness of the ratings. Pollack's results indicate that, at least for pitches, this intuition is fairly sound.

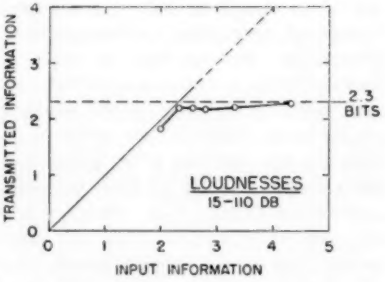

FIG. 2. Data from Garner (7) on the channel capacity for absolute judgments of auditory loudness.

Next you can ask how reproducible this result is. Does it depend on the spacing of the tones or the various conditions of judgment? Pollack varied these conditions in a number of ways. The range of frequencies can be changed by a factor of about 20 without changing the amount of information transmitted more than a small percentage. Different groupings of the pitches decreased the transmission, but the loss was small. For example, if you can discriminate five high-pitched tones in one series and five low-pitched tones in another series, it is reasonable to expect that you could combine all ten into a single series and still tell them all apart without error. When you try it, however, it does not work. The channel capacity for pitch seems to be about six and that is the best you can do.

While we are on tones, let us look next at Garner's (7) work on loudness. Garner's data for loudness are summarized in Fig. 2. Garner went to some trouble to get the best possible spacing of his tones over the intensity range from 15 to 110 db. He used 4, 5, 6, 7, 10, and 20 different stimulus intensities. The results shown in Fig. 2 take into account the differences among subjects and the sequential influence of the immediately preceding judgment. Again we find that there seems to be a limit.

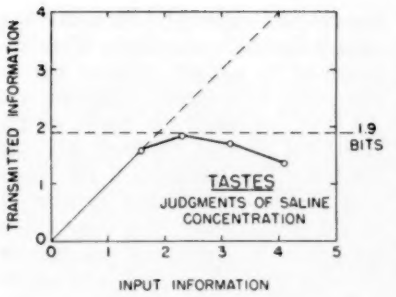

FIG. 3. Data from Beebe-Center, Rogers, and O'Connell (1) on the channel capacity for absolute judgments of saltiness.

The channel capacity for absolute judgments of loudness is 2.3 bits, or about five perfectly discriminable alternatives.

Since these two studies were done in different laboratories with slightly different techniques and methods of analysis, we are not in a good position to argue whether five loudnesses is significantly different from six pitches. Probably the difference is in the right direction, and absolute judgments of pitch are slightly more accurate than absolute judgments of loudness. The important point, however, is that the two answers are of the same order of magnitude.

The experiment has also been done for taste intensities. In Fig. 3 are the results obtained by Beebe-Center, Rogers, and O'Connell (1) for absolute judgments of the concentration of salt solutions. The concentrations ranged from 0.3 to 34.7 gm. NaCl per 100 cc. tap water in equal subjective steps. They used 3, 5, 9, and 17 different concentrations. The channel capacity is 1.9 bits, which is about four distinct concentrations. Thus taste intensities seem a little less distinctive than auditory stimuli, but again the order of magnitude is not far off.

On the other hand, the channel capacity for judgments of visual position seems to be significantly larger. Hake and Garner (8) asked observers to interpolate visually between two scale markers. Their results are shown in Fig. 4. They did the experiment in two ways. In one version they let the observer use any number between zero and 100 to describe the position, although they presented stimuli at only 5, 10, 20, or 50 different positions. The results with this unlimited response technique are shown by the filled circles on the graph. In the other version the observers were limited in their responses to reporting just those stimulus values that were possible. That is to say, in the second version the number of different responses that the observer could make was exactly the same as the number of different stimuli that the experimenter might present. The results with this limited response technique are shown by the open circles on the graph. The two functions are so similar that it seems fair to conclude that the number of responses available to the observer had nothing to do with the channel capacity of 3.25 bits.

The Hake-Garner experiment has been repeated by Coonan and Klemmer. Although they have not yet published their results, they have given me permission to say that they obtained channel capacities ranging from 3.2 bits for

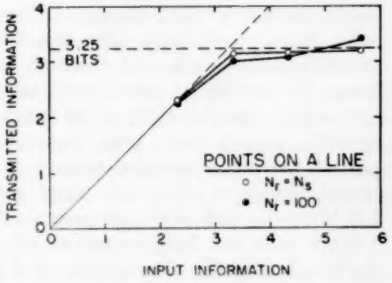

FIG. 4. Data from Hake and Garner (8) on the channel capacity for absolute judgments of the position of a pointer in a linear interval.

very short exposures of the pointer position to 3.9 bits for longer exposures. These values are slightly higher than Hake and Garner's, so we must conclude that there are between 10 and 15 distinct positions along a linear interval. This is the largest channel capacity that has been measured for any unidimensional variable.

At the present time these four experiments on absolute judgments of simple, unidimensional stimuli are all that have appeared in the psychological journals. However, a great deal of work on other stimulus variables has not yet appeared in the journals. For example, Eriksen and Hake (6) have found that the channel capacity for judging the sizes of squares is 2.2 bits, or about five categories, under a wide range of experimental conditions. In a separate experiment Eriksen (5) found 2.8 bits for size, 3.1 bits for hue, and 2.3 bits for brightness. Geldard has measured the channel capacity for the skin by placing vibrators on the chest region. A good observer can identify about four intensities, about five durations, and about seven locations.

One of the most active groups in this area has been the Air Force Operational Applications Laboratory. Pollack has been kind enough to furnish me with the results of their measurements for several aspects of visual displays. They made measurements for area and for the curvature, length, and direction of lines. In one set of experiments they used a very short exposure of the stimulus—$\frac{1}{10}$ second—and then they repeated the measurements with a 5-second exposure. For area they got 2.6 bits with the short exposure and 2.7 bits with the long exposure. For the length of a line they got about 2.6 bits with the short exposure and about 3.0 bits with the long exposure. Direction, or angle of inclination, gave 2.8 bits for the short exposure and 3.3 bits for the long exposure. Curvature was apparently harder to judge. When the length of the arc was constant, the result at the short exposure duration was 2.2 bits, but when the length of the chord was constant, the result was only 1.6 bits. This last value is the lowest that anyone has measured to date. I should add, however, that these values are apt to be slightly too low because the data from all subjects were pooled before the transmitted information was computed.

Now let us see where we are. First, the channel capacity does seem to be a valid notion for describing human observers. Second, the channel capacities measured for these unidimensional variables range from 1.6 bits for curvature to 3.9 bits for positions in an interval. Although there is no question that the differences among the variables are real and meaningful, the more impressive fact to me is their considerable similarity. If I take the best estimates I can get of the channel capacities for all the stimulus variables I have mentioned, the mean is 2.6 bits and the standard deviation is only 0.6 bit. In terms of distinguishable alternatives, this mean corresponds to about 6.5 categories, one standard deviation includes from 4 to 10 categories, and the total range is from 3 to 15 categories. Considering the wide variety of different variables that have been studied, I find this to be a remarkably narrow range.

There seems to be some limitation built into us either by learning or by the design of our nervous systems, a limit that keeps our channel capacities in this general range. On the basis of the present evidence it seems safe to say that we possess a finite and rather small capacity for making such unidimensional judgments and that this capacity does not vary a great deal from one simple sensory attribute to another.

ABSOLUTE JUDGMENTS OF MULTI-DIMENSIONAL STIMULI

You may have noticed that I have been careful to say that this magical number seven applies to one-dimensional judgments. Everyday experience teaches us that we can identify accurately any one of several hundred faces, any one of several thousand words, any one of several thousand objects, etc. The story certainly would not be complete if we stopped at this point. We must have some understanding of why the one-dimensional variables we judge in the laboratory give results so far out of line with what we do constantly in our behavior outside the laboratory. A possible explanation lies in the number of independently variable attributes of the stimuli that are being judged. Objects, faces, words, and the like differ from one another in many ways, whereas the simple stimuli we have considered thus far differ from one another in only one respect.

Fortunately, there are a few data on what happens when we make absolute judgments of stimuli that differ from one another in several ways. Let us look first at the results Klemmer and Frick (13) have reported for the absolute judgment of the position of a dot in a square. In Fig. 5 we see their results. Now the channel capacity seems to have increased to 4.6 bits, which means that people can identify accurately any one of 24 positions in the square.

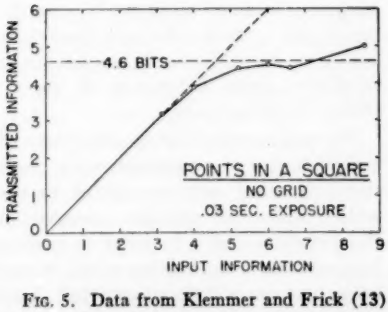

FIG. 5. Data from Klemmer and Frick (13) on the channel capacity for absolute judgments of the position of a dot in a square.

The position of a dot in a square is clearly a two-dimensional proposition. Both its horizontal and its vertical position must be identified. Thus it seems natural to compare the 4.6-bit capacity for a square with the 3.25-bit capacity for the position of a point in an interval. The point in the square requires two judgments of the interval type. If we have a capacity of 3.25 bits for estimating intervals and we do this twice, we should get 6.5 bits as our capacity for locating points in a square. Adding the second independent dimension gives us an increase from 3.25 to 4.6, but it falls short of the perfect addition that would give 6.5 bits.

Another example is provided by Beebe-Center, Rogers, and O'Connell. When they asked people to identify both the saltiness and the sweetness of solutions containing various concentrations of salt and sucrose, they found that the channel capacity was 2.3 bits. Since the capacity for salt alone was 1.9, we might expect about 3.8 bits if the two aspects of the compound stimuli were judged independently. As with spatial locations, the second dimension adds a little to the capacity but not as much as it conceivably might.

A third example is provided by Pollack (18), who asked listeners to judge both the loudness and the pitch of pure tones. Since pitch gives 2.5 bits and loudness gives 2.3 bits, we might hope to get as much as 4.8 bits for pitch and loudness together. Pollack obtained 3.1 bits, which again indicates that the second dimension augments the channel capacity but not so much as it might.

A fourth example can be drawn from the work of Halsey and Chapanis (9) on confusions among colors of equal

luminance. Although they did not analyze their results in informational terms, they estimate that there are about 11 to 15 identifiable colors, or, in our terms, about 3.6 bits. Since these colors varied in both hue and saturation, it is probably correct to regard this as a two-dimensional judgment. If we compare this with Eriksen's 3.1 bits for hue (which is a questionable comparison to draw), we again have something less than perfect addition when a second dimension is added.

It is still a long way, however, from these two-dimensional examples to the multidimensional stimuli provided by faces, words, etc. To fill this gap we have only one experiment, an auditory study done by Pollack and Ficks (19). They managed to get six different acoustic variables that they could change: frequency, intensity, rate of interruption, on-time fraction, total duration, and spatial location. Each one of these six variables could assume any one of five different values, so altogether there were 5^6, or 15,625 different tones that they could present. The listeners made a separate rating for each one of these six dimensions. Under these conditions the transmitted information was 7.2 bits, which corresponds to about 150 different categories that could be absolutely identified without error. Now we are beginning to get up into the range that ordinary experience would lead us to expect.

Suppose that we plot these data, fragmentary as they are, and make a guess about how the channel capacity changes with the dimensionality of the stimuli. The result is given in Fig. 6. In a moment of considerable daring I sketched the dotted line to indicate roughly the trend that the data seemed to be taking.

Clearly, the addition of independently variable attributes to the stimulus increases the channel capacity, but at a

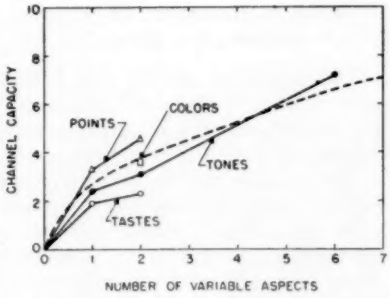

FIG. 6. The general form of the relation between channel capacity and the number of independently variable attributes of the stimuli.

decreasing rate. It is interesting to note that the channel capacity is increased even when the several variables are not independent. Eriksen (5) reports that, when size, brightness, and hue all vary together in perfect correlation, the transmitted information is 4.1 bits as compared with an average of about 2.7 bits when these attributes are varied one at a time. By confounding three attributes, Eriksen increased the dimensionality of the input without increasing the amount of input information; the result was an increase in channel capacity of about the amount that the dotted function in Fig. 6 would lead us to expect.

The point seems to be that, as we add more variables to the display, we increase the total capacity, but we decrease the accuracy for any particular variable. In other words, we can make relatively crude judgments of several things simultaneously.

We might argue that in the course of evolution those organisms were most successful that were responsive to the widest range of stimulus energies in their environment. In order to survive in a constantly fluctuating world, it was better to have a little information about a lot of things than to have a lot of information about a small segment of the

environment. If a compromise was necessary, the one we seem to have made is clearly the more adaptive.

Pollack and Ficks's results are very strongly suggestive of an argument that linguists and phoneticians have been making for some time (11). According to the linguistic analysis of the sounds of human speech, there are about eight or ten dimensions—the linguists call them *distinctive features*—that distinguish one phoneme from another. These distinctive features are usually binary, or at most ternary, in nature. For example, a binary distinction is made between vowels and consonants, a binary decision is made between oral and nasal consonants, a ternary decision is made among front, middle, and back phonemes, etc. This approach gives us quite a different picture of speech perception than we might otherwise obtain from our studies of the speech spectrum and of the ear's ability to discriminate relative differences among pure tones. I am personally much interested in this new approach (15), and I regret that there is not time to discuss it here.

It was probably with this linguistic theory in mind that Pollack and Ficks conducted a test on a set of tonal stimuli that varied in eight dimensions, but required only a binary decision on each dimension. With these tones they measured the transmitted information at 6.9 bits, or about 120 recognizable kinds of sounds. It is an intriguing question, as yet unexplored, whether one can go on adding dimensions indefinitely in this way.

In human speech there is clearly a limit to the number of dimensions that we use. In this instance, however, it is not known whether the limit is imposed by the nature of the perceptual machinery that must recognize the sounds or by the nature of the speech machinery that must produce them. Somebody will have to do the experiment to find out. There is a limit, however, at about eight or nine distinctive features in every language that has been studied, and so when we talk we must resort to still another trick for increasing our channel capacity. Language uses sequences of phonemes, so we make several judgments successively when we listen to words and sentences. That is to say, we use both simultaneous and successive discriminations in order to expand the rather rigid limits imposed by the inaccuracy of our absolute judgments of simple magnitudes.

These multidimensional judgments are strongly reminiscent of the abstraction experiment of Külpe (14). As you may remember, Külpe showed that observers report more accurately on an attribute for which they are set than on attributes for which they are not set. For example, Chapman (4) used three different attributes and compared the results obtained when the observers were instructed before the tachistoscopic presentation with the results obtained when they were not told until after the presentation which one of the three attributes was to be reported. When the instruction was given in advance, the judgments were more accurate. When the instruction was given afterwards, the subjects presumably had to judge all three attributes in order to report on any one of them and the accuracy was correspondingly lower. This is in complete accord with the results we have just been considering, where the accuracy of judgment on each attribute decreased as more dimensions were added. The point is probably obvious, but I shall make it anyhow, that the abstraction experiments did *not* demonstrate that people can judge only one attribute at a time. They merely showed what seems quite reasonable, that people are less accurate if they must judge more than one attribute simultaneously.

SUBITIZING

I cannot leave this general area without mentioning, however briefly, the experiments conducted at Mount Holyoke College on the discrimination of number (12). In experiments by Kaufman, Lord, Reese, and Volkmann random patterns of dots were flashed on a screen for 1/5 of a second. Anywhere from 1 to more than 200 dots could appear in the pattern. The subject's task was to report how many dots there were.

The first point to note is that on patterns containing up to five or six dots the subjects simply did not make errors. The performance on these small numbers of dots was so different from the performance with more dots that it was given a special name. Below seven the subjects were said to *subitize;* above seven they were said to *estimate.* This is, as you will recognize, what we once optimistically called "the span of attention."

This discontinuity at seven is, of course, suggestive. Is this the same basic process that limits our unidimensional judgments to about seven categories? The generalization is tempting, but not sound in my opinion. The data on number estimates have not been analyzed in informational terms; but on the basis of the published data I would guess that the subjects transmitted something more than four bits of information about the number of dots. Using the same arguments as before, we would conclude that there are about 20 or 30 distinguishable categories of numerousness. This is considerably more information than we would expect to get from a unidimensional display. It is, as a matter of fact, very much like a two-dimensional display. Although the dimensionality of the random dot patterns is not entirely clear, these results are in the same range as Klemmer and Frick's for their two-dimensional display of dots in a square. Perhaps the two dimensions of numerousness are area and density. When the subject can subitize, area and density may not be the significant variables, but when the subject must estimate perhaps they are significant. In any event, the comparison is not so simple as it might seem at first thought.

This is one of the ways in which the magical number seven has persecuted me. Here we have two closely related kinds of experiments, both of which point to the significance of the number seven as a limit on our capacities. And yet when we examine the matter more closely, there seems to be a reasonable suspicion that it is nothing more than a coincidence.

THE SPAN OF IMMEDIATE MEMORY

Let me summarize the situation in this way. There is a clear and definite limit to the accuracy with which we can identify absolutely the magnitude of a unidimensional stimulus variable. I would propose to call this limit the *span of absolute judgment,* and I maintain that for unidimensional judgments this span is usually somewhere in the neighborhood of seven. We are not completely at the mercy of this limited span, however, because we have a variety of techniques for getting around it and increasing the accuracy of our judgments. The three most important of these devices are (*a*) to make relative rather than absolute judgments; or, if that is not possible, (*b*) to increase the number of dimensions along which the stimuli can differ; or (*c*) to arrange the task in such a way that we make a sequence of several absolute judgments in a row.

The study of relative judgments is one of the oldest topics in experimental psychology, and I will not pause to review it now. The second device, increasing the dimensionality, we have just considered. It seems that by adding

more dimensions and requiring crude, binary, yes-no judgments on each attribute we can extend the span of absolute judgment from seven to at least 150. Judging from our everyday behavior, the limit is probably in the thousands, if indeed there is a limit. In my opinion, we cannot go on compounding dimensions indefinitely. I suspect that there is also a *span of perceptual dimensionality* and that this span is somewhere in the neighborhood of ten, but I must add at once that there is no objective evidence to support this suspicion. This is a question sadly needing experimental exploration.

Concerning the third device, the use of successive judgments, I have quite a bit to say because this device introduces memory as the handmaiden of discrimination. And, since mnemonic processes are at least as complex as are perceptual processes, we can anticipate that their interactions will not be easily disentangled.

Suppose that we start by simply extending slightly the experimental procedure that we have been using. Up to this point we have presented a single stimulus and asked the observer to name it immediately thereafter. We can extend this procedure by requiring the observer to withhold his response until we have given him several stimuli in succession. At the end of the sequence of stimuli he then makes his response. We still have the same sort of input-output situation that is required for the measurement of transmitted information. But now we have passed from an experiment on absolute judgment to what is traditionally called an experiment on immediate memory.

Before we look at any data on this topic I feel I must give you a word of warning to help you avoid some obvious associations that can be confusing. Everybody knows that there is a finite span of immediate memory and that for a lot of different kinds of test materials this span is about seven items in length. I have just shown you that there is a span of absolute judgment that can distinguish about seven categories and that there is a span of attention that will encompass about six objects at a glance. What is more natural than to think that all three of these spans are different aspects of a single underlying process? And that is a fundamental mistake, as I shall be at some pains to demonstrate. This mistake is one of the malicious persecutions that the magical number seven has subjected me to.

My mistake went something like this. We have seen that the invariant feature in the span of absolute judgment is the amount of information that the observer can transmit. There is a real operational similarity between the absolute judgment experiment and the immediate memory experiment. If immediate memory is like absolute judgment, then it should follow that the invariant feature in the span of immediate memory is also the amount of information that an observer can retain. If the amount of information in the span of immediate memory is a constant, then the span should be short when the individual items contain a lot of information and the span should be long when the items contain little information. For example, decimal digits are worth 3.3 bits apiece. We can recall about seven of them, for a total of 23 bits of information. Isolated English words are worth about 10 bits apiece. If the total amount of information is to remain constant at 23 bits, then we should be able to remember only two or three words chosen at random. In this way I generated a theory about how the span of immediate memory should vary as a function of the amount of information per item in the test materials.

The measurements of memory span in the literature are suggestive on this

question, but not definitive. And so it was necessary to do the experiment to see. Hayes (10) tried it out with five different kinds of test materials: binary digits, decimal digits, letters of the alphabet, letters plus decimal digits, and with 1,000 monosyllabic words. The lists were read aloud at the rate of one item per second and the subjects had as much time as they needed to give their responses. A procedure described by Woodworth (20) was used to score the responses.

The results are shown by the filled circles in Fig. 7. Here the dotted line indicates what the span should have been if the amount of information in the span were constant. The solid curves represent the data. Hayes repeated the experiment using test vocabularies of different sizes but all containing only English monosyllables (open circles in Fig. 7). This more homogeneous test material did not change the picture significantly. With binary items the span is about nine and, although it drops to about five with monosyllabic English words, the difference is far less than the hypothesis of constant information would require.

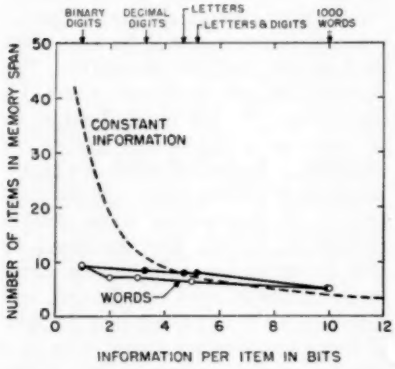

FIG. 7. Data from Hayes (10) on the span of immediate memory plotted as a function of the amount of information per item in the test materials.

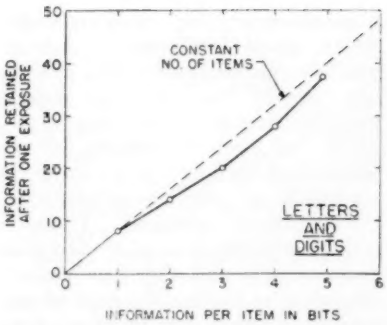

FIG. 8. Data from Pollack (16) on the amount of information retained after one presentation plotted as a function of the amount of information per item in the test materials.

There is nothing wrong with Hayes's experiment, because Pollack (16) repeated it much more elaborately and got essentially the same result. Pollack took pains to measure the amount of information transmitted and did not rely on the traditional procedure for scoring the responses. His results are plotted in Fig. 8. Here it is clear that the amount of information transmitted is not a constant, but increases almost linearly as the amount of information per item in the input is increased.

And so the outcome is perfectly clear. In spite of the coincidence that the magical number seven appears in both places, the span of absolute judgment and the span of immediate memory are quite different kinds of limitations that are imposed on our ability to process information. Absolute judgment is limited by the amount of information. Immediate memory is limited by the number of items. In order to capture this distinction in somewhat picturesque terms, I have fallen into the custom of distinguishing between *bits* of information and *chunks* of information. Then I can say that the number of bits of information is constant for absolute judgment and the number of chunks of informa-

tion is constant for immediate memory. The span of immediate memory seems to be almost independent of the number of bits per chunk, at least over the range that has been examined to date.

The contrast of the terms *bit* and *chunk* also serves to highlight the fact that we are not very definite about what constitutes a chunk of information. For example, the memory span of five words that Hayes obtained when each word was drawn at random from a set of 1000 English monosyllables might just as appropriately have been called a memory span of 15 phonemes, since each word had about three phonemes in it. Intuitively, it is clear that the subjects were recalling five words, not 15 phonemes, but the logical distinction is not immediately apparent. We are dealing here with a process of organizing or grouping the input into familiar units or chunks, and a great deal of learning has gone into the formation of these familiar units.

RECODING

In order to speak more precisely, therefore, we must recognize the importance of grouping or organizing the input sequence into units or chunks. Since the memory span is a fixed number of chunks, we can increase the number of bits of information that it contains simply by building larger and larger chunks, each chunk containing more information than before.

A man just beginning to learn radiotelegraphic code hears each *dit* and *dah* as a separate chunk. Soon he is able to organize these sounds into letters and then he can deal with the letters as chunks. Then the letters organize themselves as words, which are still larger chunks, and he begins to hear whole phrases. I do not mean that each step is a discrete process, or that plateaus must appear in his learning curve, for surely the levels of organization are achieved at different rates and overlap each other during the learning process. I am simply pointing to the obvious fact that the dits and dahs are organized by learning into patterns and that as these larger chunks emerge the amount of message that the operator can remember increases correspondingly. In the terms I am proposing to use, the operator learns to increase the bits per chunk.

In the jargon of communication theory, this process would be called *recoding*. The input is given in a code that contains many chunks with few bits per chunk. The operator recodes the input into another code that contains fewer chunks with more bits per chunk. There are many ways to do this recoding, but probably the simplest is to group the input events, apply a new name to the group, and then remember the new name rather than the original input events.

Since I am convinced that this process is a very general and important one for psychology, I want to tell you about a demonstration experiment that should make perfectly explicit what I am talking about. This experiment was conducted by Sidney Smith and was reported by him before the Eastern Psychological Association in 1954.

Begin with the observed fact that people can repeat back eight decimal digits, but only nine binary digits. Since there is a large discrepancy in the amount of information recalled in these two cases, we suspect at once that a recoding procedure could be used to increase the span of immediate memory for binary digits. In Table 1 a method for grouping and renaming is illustrated. Along the top is a sequence of 18 binary digits, far more than any subject was able to recall after a single presentation. In the next line these same binary digits are grouped by pairs. Four possible pairs can occur: 00 is renamed 0, 01 is renamed 1, 10 is renamed 2, and 11 is

TABLE 1

WAYS OF RECODING SEQUENCES OF BINARY DIGITS

Binary Digits (Bits)		1	0	1	0	0	0	1	0	0	1	1	1	0	0	1	1	1	0
2:1	Chunks	10		10		00		10		01		11		00		11		10	
	Recoding	2		2		0		2		1		3		0		3		2	
3:1	Chunks	101			000			100			111			001			110		
	Recoding	5			0			4			7			1			6		
4:1	Chunks	1010				0010				0111				0011				10	
	Recoding	10				2				7				3					
5:1	Chunks	10100					01001					11001						110	
	Recoding	20					9					25							

renamed 3. That is to say, we recode from a base-two arithmetic to a base-four arithmetic. In the recoded sequence there are now just nine digits to remember, and this is almost within the span of immediate memory. In the next line the same sequence of binary digits is regrouped into chunks of three. There are eight possible sequences of three, so we give each sequence a new name between 0 and 7. Now we have recoded from a sequence of 18 binary digits into a sequence of 6 octal digits, and this is well within the span of immediate memory. In the last two lines the binary digits are grouped by fours and by fives and are given decimal-digit names from 0 to 15 and from 0 to 31.

It is reasonably obvious that this kind of recoding increases the bits per chunk, and packages the binary sequence into a form that can be retained within the span of immediate memory. So Smith assembled 20 subjects and measured their spans for binary and octal digits. The spans were 9 for binaries and 7 for octals. Then he gave each recoding scheme to five of the subjects. They studied the recoding until they said they understood it—for about 5 or 10 minutes. Then he tested their span for binary digits again while they tried to use the recoding schemes they had studied.

The recoding schemes increased their span for binary digits in every case. But the increase was not as large as we had expected on the basis of their span for octal digits. Since the discrepancy increased as the recoding ratio increased, we reasoned that the few minutes the subjects had spent learning the recoding schemes had not been sufficient. Apparently the translation from one code to the other must be almost automatic or the subject will lose part of the next group while he is trying to remember the translation of the last group.

Since the 4:1 and 5:1 ratios require considerable study, Smith decided to imitate Ebbinghaus and do the experiment on himself. With Germanic patience he drilled himself on each recoding successively, and obtained the results shown in Fig. 9. Here the data follow along rather nicely with the results you would predict on the basis of his span for octal digits. He could remember 12 octal digits. With the 2:1 recoding, these 12 chunks were worth 24 binary digits. With the 3:1 recoding they were worth 36 binary digits. With the 4:1 and 5:1 recodings, they were worth about 40 binary digits.

It is a little dramatic to watch a person get 40 binary digits in a row and then repeat them back without error. However, if you think of this merely as

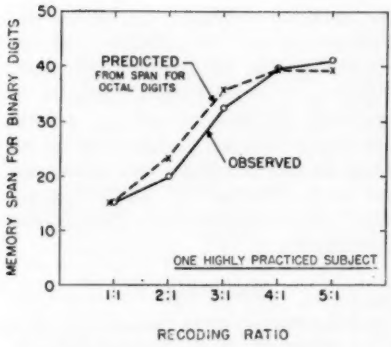

FIG. 9. The span of immediate memory for binary digits is plotted as a function of the recoding procedure used. The predicted function is obtained by multiplying the span for octals by 2, 3 and 3.3 for recoding into base 4, base 8, and base 10, respectively.

a mnemonic trick for extending the memory span, you will miss the more important point that is implicit in nearly all such mnemonic devices. The point is that recoding is an extremely powerful weapon for increasing the amount of information that we can deal with. In one form or another we use recoding constantly in our daily behavior.

In my opinion the most customary kind of recoding that we do all the time is to translate into a verbal code. When there is a story or an argument or an idea that we want to remember, we usually try to rephrase it "in our own words." When we witness some event we want to remember, we make a verbal description of the event and then remember our verbalization. Upon recall we recreate by secondary elaboration the details that seem consistent with the particular verbal recoding we happen to have made. The well-known experiment by Carmichael, Hogan, and Walter (3) on the influence that names have on the recall of visual figures is one demonstration of the process.

The inaccuracy of the testimony of eyewitnesses is well known in legal psychology, but the distortions of testimony are not random—they follow naturally from the particular recoding that the witness used, and the particular recoding he used depends upon his whole life history. Our language is tremendously useful for repackaging material into a few chunks rich in information. I suspect that imagery is a form of recoding, too, but images seem much harder to get at operationally and to study experimentally than the more symbolic kinds of recoding.

It seems probable that even memorization can be studied in these terms. The process of memorizing may be simply the formation of chunks, or groups of items that go together, until there are few enough chunks so that we can recall all the items. The work by Bousfield and Cohen (2) on the occurrence of clustering in the recall of words is especially interesting in this respect.

SUMMARY

I have come to the end of the data that I wanted to present, so I would like now to make some summarizing remarks.

First, the span of absolute judgment and the span of immediate memory impose severe limitations on the amount of information that we are able to receive, process, and remember. By organizing the stimulus input simultaneously into several dimensions and successively into a sequence of chunks, we manage to break (or at least stretch) this informational bottleneck.

Second, the process of recoding is a very important one in human psychology and deserves much more explicit attention than it has received. In particular, the kind of linguistic recoding that people do seems to me to be the very lifeblood of the thought processes. Recoding procedures are a constant concern to clinicians, social psycholo-

gists, linguists, and anthropologists and yet, probably because recoding is less accessible to experimental manipulation than nonsense syllables or **T** mazes, the traditional experimental psychologist has contributed little or nothing to their analysis. Nevertheless, experimental techniques can be used, methods of recoding can be specified, behavioral indicants can be found. And I anticipate that we will find a very orderly set of relations describing what now seems an uncharted wilderness of individual differences.

Third, the concepts and measures provided by the theory of information provide a quantitative way of getting at some of these questions. The theory provides us with a yardstick for calibrating our stimulus materials and for measuring the performance of our subjects. In the interests of communication I have suppressed the technical details of information measurement and have tried to express the ideas in more familiar terms; I hope this paraphrase will not lead you to think they are not useful in research. Informational concepts have already proved valuable in the study of discrimination and of language; they promise a great deal in the study of learning and memory; and it has even been proposed that they can be useful in the study of concept formation. A lot of questions that seemed fruitless twenty or thirty years ago may now be worth another look. In fact, I feel that my story here must stop just as it begins to get really interesting.

And finally, what about the magical number seven? What about the seven wonders of the world, the seven seas, the seven deadly sins, the seven daughters of Atlas in the Pleiades, the seven ages of man, the seven levels of hell, the seven primary colors, the seven notes of the musical scale, and the seven days of the week? What about the seven-point rating scale, the seven categories for absolute judgment, the seven objects in the span of attention, and the seven digits in the span of immediate memory? For the present I propose to withhold judgment. Perhaps there is something deep and profound behind all these sevens, something just calling out for us to discover it. But I suspect that it is only a pernicious, Pythagorean coincidence.

REFERENCES

1. BEEBE-CENTER, J. G., ROGERS, M. S., & O'CONNELL, D. N. Transmission of information about sucrose and saline solutions through the sense of taste. *J. Psychol.*, 1955, **39**, 157–160.
2. BOUSFIELD, W. A., & COHEN, B. H. The occurrence of clustering in the recall of randomly arranged words of different frequencies-of-usage. *J. gen. Psychol.*, 1955, **52**, 83–95.
3. CARMICHAEL, L., HOGAN, H. P., & WALTER, A. A. An experimental study of the effect of language on the reproduction of visually perceived form. *J. exp. Psychol.*, 1932, **15**, 73–86.
4. CHAPMAN, D. W. Relative effects of determinate and indeterminate *Aufgaben*. *Amer. J. Psychol.*, 1932, **44**, 163–174.
5. ERIKSEN, C. W. Multidimensional stimulus differences and accuracy of discrimination. *USAF, WADC Tech. Rep.*, 1954, No. 54–165.
6. ERIKSEN, C. W., & HAKE, H. W. Absolute judgments as a function of the stimulus range and the number of stimulus and response categories. *J. exp. Psychol.*, 1955, **49**, 323–332.
7. GARNER, W. R. An informational analysis of absolute judgments of loudness. *J. exp. Psychol.*, 1953, **46**, 373–380.
8. HAKE, H. W., & GARNER, W. R. The effect of presenting various numbers of discrete steps on scale reading accuracy. *J. exp. Psychol.*, 1951, **42**, 358–366.
9. HALSEY, R. M., & CHAPANIS, A. Chromaticity-confusion contours in a complex viewing situation. *J. Opt. Soc. Amer.*, 1954, **44**, 442–454.
10. HAYES, J. R. M. Memory span for several vocabularies as a function of vocabulary size. In *Quarterly Progress Report*, Cambridge, Mass.: Acoustics Laboratory, Massachusetts Institute of Technology, Jan.–June, 1952.

11. JAKOBSON, R., FANT, C. G. M., & HALLE, M. *Preliminaries to speech analysis.* Cambridge, Mass.: Acoustics Laboratory, Massachusetts Institute of Technology, 1952. (Tech. Rep. No. 13.)
12. KAUFMAN, E. L., LORD, M. W., REESE, T. W., & VOLKMANN, J. The discrimination of visual number. *Amer. J. Psychol.*, 1949, **62**, 498–525.
13. KLEMMER, E. T., & FRICK, F. C. Assimilation of information from dot and matrix patterns. *J. exp. Psychol.*, 1953, **45**, 15–19.
14. KÜLPE, O. Versuche über Abstraktion. *Ber. ü. d. I Kongr. f. exper. Psychol.*, 1904, 56–68.
15. MILLER, G. A., & NICELY, P. E. An analysis of perceptual confusions among some English consonants. *J. Acoust. Soc. Amer.*, 1955, **27**, 338–352.
16. POLLACK, I. The assimilation of sequentially encoded information. *Amer. J. Psychol.*, 1953, **66**, 421–435.
17. POLLACK, I. The information of elementary auditory displays. *J. Acoust. Soc. Amer.*, 1952, **24**, 745–749.
18. POLLACK, I. The information of elementary auditory displays. II. *J. Acoust. Soc. Amer.*, 1953, **25**, 765–769.
19. POLLACK, I., & FICKS, L. Information of elementary multi-dimensional auditory displays. *J. Acoust. Soc. Amer.*, 1954, **26**, 155–158.
20. WOODWORTH, R. S. *Experimental psychology.* New York: Holt, 1938.

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CONFIRMATION OF THEORIES IN PSYCHOLOGY

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This paper is an inquiry into the methodology of confirming theories in psychology. By *theory* we shall mean any empirical generalization—a hypothesis, a law, a postulate, etc. The procedure currently used for testing theories is known as *inference by confirmation*, which is an invalid procedure. To illustrate the fallacy involved, consider a simple statement of a theory: $a \supset b$;[3] for example, let a stand for the proposition, "an industrial work group is in great inner conflict," and b for "that work group has a low production level." The formula may then be interpreted thus: "*if* an industrial work group is in great inner conflict, *then* that work group has a low production level." Normally, to test this theory we would measure the work output of such a group. If we find that it *is* low, compared to a control group, we would assert the consequent of the theory, that is, b. A probability inference from b to the theory, $a \supset b$, would then be made and the conclusion would be reached that the probability of the theory is increased. However, when the consequent is found to be true, it does not follow that the probability of the theory is increased; alternative theories of psychology may also imply b; for example, $a' \supset b$, $a'' \supset b$, and so on (cf. **2**). As Reichenbach (**12**) shows, when it seems that a theory is confirmed *strictly* by its consequents, more is really known than is explicitly stated; other probabilities, to be discussed later, are necessary for a valid inference.

This fallacy has been variously pointed out by psychologists. For example, Miller says that Hull ". . . is wrong in saying that . . . postulates can receive indirect verification . . . [for] two incorrect postulates in a system may give rise to a correct conclusion which is directly verifiable. . . ." He adds that one cannot argue from observed conclusions to a proof of premises, for this is the "fallacy of affirming the consequent" (**11**, p. 468). Similarly, Johnson says:

> . . . to argue to the truth of an hypothesis by affirming the consequents is always a fallacy no matter how many consequents are affirmed. . . . The verification or disverification of the implicates is important and useful [for] its own account, for by it we discover something about the world in which we live, but the verification of the implicates does not establish the truth of the hypothesis (**9**, p. 727).

When Seeman (**14**) seeks to make an "inductive leap" from the consequent to the theory, Behan and Behan (**1**) comment that the "inductive leap" amounts to the fallacy of asserting the consequent, an operation excluded in symbolic logic. Seeman replies:

> To call this "asserting the consequent" is to say either that all experimental procedure leads to this fallacy or to assert a stricture against all empirical science. . . . What purpose, after all, can an experiment serve if, from the results, one is forbidden to make inferences?" (**15**, p. 178).

Here we truly have an important problem that must be faced and solved.

A part of the problem as summarized above results from a confusion of the different functions of inductive and deductive logic. As it is applicable here,

[1] Now at Hollins College.

[2] The author is indebted to W. Battrick, R. Carnap, J. E. Freund, F. George, E. F. MacCaslin, P. Oppenheim, and W. Salmon, for criticizing earlier drafts of this paper, although they are not responsible for its final form.

[3] The symbol "$\supset$" is the notation for the logical operation of implication and may be read "*if* . . . *then*. . . ."

deductive logic is used in deriving more specific propositions (here, generally consequents) from more general ones (here, theories); whereas inductive logic is used in ascertaining the probability value of the more general propositions on the basis of the more specific ones. All the criticisms referred to above hold that when inference by confirmation is used, the fallacy of affirming the consequent is being committed. However, this argument is incorrect because the fallacy of affirming the consequent is a fallacy of deductive logic, and in this situation we are concerned with inductive logic. The more proper name is the *fallacy of incomplete schematization*—and the fact remains that it is an erroneous procedure; nowhere in the calculus of probability, and thus in inductive logic, may such an inference be found.

We thus are faced with a vital question that needs to be answered before psychology can progress to an advanced state of knowledge, i.e., if inference by confirmation is an invalid procedure, what is a valid one by which we can test our theories? The answer to this question will serve at least two main functions: first, we will have available objective specifications which will allow us to agree as to whether a given theory is confirmed or disconfirmed, rather than relying on the intuitive appraisal of the individual scientist; and second, we can discriminate between meaningful and meaningless propositions, since a proposition is meaningful if and only if it is possible to obtain the kind of data that would be relevant to the confirmation or disconfirmation of it. There is no point in carrying out a large number of theory-oriented experiments if we cannot make valid inferences back to the theories which stimulated them. Hempel says that "The establishment of a general theory of confirmation may well be regarded as one of the most urgent desiderata of the present methodology of empirical science" (**8**, p. 3). That this is not an easy problem, however, is apparent from the diverse approaches and discordant views in the literature (e.g., **3**, **4**, **5**, **7**, **8**, **12**, **13**). Realizing that anything else would be premature, we will present here a tentative solution, particularly with reference to psychology.

TESTING THE THEORY

The first step in testing a theory is to summarize the data of a given experiment in the form of an *evidence report* (*E*). An evidence report is a proposition which says that, when specified conditions obtain, a given entity either has or does not have, a certain attribute; it can therefore be either positive or negative. For instance, in the example previously cited, a positive evidence report would read: "x is an industrial work group in great inner conflict, and x has a low production level." The precise method of arriving at an evidence report is not germane to our discussion, and therefore will not be elaborated here. The main problem is to confront the theory with the evidence report and determine whether the theory is confirmed or disconfirmed.

Strictly Verifiable Theories

It can be shown that under certain circumstances it is possible to determine strictly the truth value of a theory solely on the basis of its consequents. We will here distinguish between the processes of verification and confirmation. Confirmation allows classification of a theory as probable or improbable, and requires that we consider a number of separate evidence reports; while verification allows classification of a theory as completely true or completely false, as based on only one evidence report. Verification, therefore, is a narrower term than confirmation, and may be included as a limiting case of confirmation where the probability is 1.0. Whether or not a theory can be verified is determined

exclusively by its logical form; in verification we use deductive logic only. A positive universal theory (e.g., $a \supset b$) is thus verifiable if it is false, but not verifiable if it is true.[4] A negative universal theory (e.g., $\overline{a \supset b}$)[5] is verifiable if it is true, but not verifiable if it is false (cf. Reichenbach [**12**, p. 341], Hempel [**8**]).

Positive Evidence Reports and Positive Universal Theories

We thus see that, under certain conditions, we can verify our theories. The heart of the problem remaining is to determine whether a positive evidence report is an instance of support of a universal theory. One promising possibility is to use inverse induction, particularly Bayes's rule (theorem), though the precise methods of its application are not available.

The inference of indirect evidence. According to Reichenbach ". . . the general inductive inference from observational data to the validity of a given scientific theory must be regarded as an inference in terms of Bayes's rule" (**12**, p. 95). In order to make a valid inductive inference, which Reichenbach terms *inference by indirect evidence*, certain other probabilities must be taken into account, all of which are expressed in Bayes's rule. This rule is derivable from the axioms of the calculus of probability, and it thus has the status of a theorem. It determines *inverse probabilities* as a function of forward probabilities (the quantities on the left and the right, respectively, of formula 1). This inverse probability is the value that we wish to determine; i.e., it will tell us the probability that a positive evidence report (E) is an instance of support of a given theory (T). The primary problems in using Bayes's rule are in interpreting the probabilities it contains, and in defining the necessary reference classes. Following a frequency interpretation (cf. **12**) we shall consider each probability as the limit of relative frequencies of positive instances among all relevant cases. Although there are no hard and fast rules for interpreting these probabilities and defining the necessary reference classes, we will suggest some tentative solutions.

Let us first attempt to define the reference classes necessary to apply Bayes's rule. A reference class comprises the universe of things with which an inquiry is concerned; specifically, it is the class of elements of a sequence of events which determines the frequency ratio. One reason why we must consider reference classes is that, when using the calculus of probability, we can speak of an individual event only insofar as that event is an element in a specified reference class. We cannot, therefore, strictly speak of the probability of a single theory, but must consider the class of theories of which it is a member. The probability of an individual event (e.g., a theory) thus has a fictitious, but useful, meaning. In applying the rule of Bayes, there are four reference classes that we must define: A, E, T and $\bar{T}$. Our aim is to develop the widest possible homogeneous reference classes.

The reference class A may be considered as the widest possible class of homogeneous theories. In other words,

[4] The calculus of propositions, a two-valued system, is being used here for stating theories. Strictly speaking, this is improper because all empirical propositions have a probability character. However, since the logical treatment of theories in a two-valued logic is simpler than in probability logic, it is desirable to use the former (cf. **10**). Reichenbach (**12**, p. 95) justifies the transition from a proposition stated in probability logic to one in a two-valued logic under certain fairly liberal conditions, with the qualification that the probability of the theory does not attain certainty. We should assume this transition throughout the remainder of this paper, for purposes of approximation; this assumption should particularly be borne in mind with reference to Table 1.

[5] A line above a symbol is the notation for the logical operation of negation; here it may be read "it is false that a implies b."

we want to include in it as many theories as possible which are *essentially similar* to the theory with which we are dealing. If it is a learning theory, perhaps the reference class A will end up as all learning theories. In order to obtain a homogeneous reference class, we want to exclude from A those theories which are *essentially different* from our specific theory. Salmon (**13**) discusses the terms *essential similarity* and *essential difference*, offers an apparently adequate method for defining them, and thus provides a criterion for establishing a class of homogeneous theories. Our reference class A may be divided into several subclasses; in our simplified case we will consider two subclasses, T and $\bar{T}$. The subclass T may refer to true theories included in A; the subclass $\bar{T}$ will refer to false theories in A. The reference class E will refer to the class of positive evidence reports. A statement of Bayes's rule is:[6]

$$P(A.E,T) = \frac{P(A,T)\cdot P(A.T,E)}{P(A,T)\cdot P(A.T,E)+P(A,\bar{T})\cdot P(A.\bar{T},E)}. \quad [1]$$

The four different probabilities included in Bayes's rule may now be interpreted as follows:

1. $P(A,T)$ is known as the *antecedent probability*, and is the probability that a theory of the type with which we are dealing is true, irrespective of the evidence report under consideration. This probability is based on the limit of the frequency of true theories (T) included in the reference class A, to all of the theories in $A(T+\bar{T})$. Reichenbach says of antecedent probabilities that they ". . . often need not be known otherwise than in the form of crude estimates, while the results of the inference can be very precise" (**12**, p. 432). Others hold similar views (e.g., **5**, **6**).

2. $P(A,\bar{T})$ is the probability that our theory is false, irrespective of the evidence report under consideration, and may be determined from formula 2:

$$P(A,\bar{T}) = 1 - P(A,T). \quad [2]$$

3. $P(A.T,E)$ is the probability that when the initial conditions of the theory obtain, the consequences occur. Here the evidence report is positive (E) and, as Reichenbach says, the probability may be considered to be 1.0. If the evidence report is negative, this probability becomes 0 (and the inverse probability in formula 1 becomes 0). The fact that this probability can take on only the values 0 and 1, in the present context, results from our use of a two-valued logic for stating theories (see footnote 2).

4. $P(A.\bar{T},E)$ is the probability of obtaining a positive evidence report under the condition that the theory is false and the statement of the evidence report is valid. This probability is part of the initial evidence; it asserts that the true value of the consequence of the theory is not affected whether or not the initial conditions of the theory hold. It may be determined by computing the ratio of positive evidence reports to all evidence reports obtained for the false theories contained in A.

Let us now consider a lattice made up of theories that we wish to include in our reference class A (see Table 1). The first row of the lattice will be composed of the positive and negative evidence reports obtained from a series of experiments designed to test theory 1; Row 2 is similarly composed of evidence reports for Theory 2; and so on for, say, five theories. Let us assume that Theories 1 through 4 are true (highly probable), while Theory 5, on the other hand is false (highly improbable).[7]

[6] The symbol "." is the notation for the logical operation of conjunction and may be read *and*. The "P symbol," e.g., $P(A.E,T)$, may be read "The probability from A and E to T."

[7] One might ask: How can we construct a lattice out of confirmed theories, when such a

TABLE 1

A LATTICE MADE UP OF POSITIVE AND NEGATIVE EVIDENCE REPORTS FOR FIVE THEORIES WHICH REFERENCE CLASS A INCLUDES

Theory	Value of Evidence Reports Based on 10 Experiments Per Theory	Approximate Truth Value of Theory
1	++++++−+++	True
2	++−++++++++	True
3	++++++++++	True
4	−+++++++++	True
5	+−−−−−+−−−	False

We are now in a position to compute the probabilities contained in Bayes's rule, and solve for the inverse probability that a positive evidence report is an instance of support of a theory belonging to the reference class A.

The class A is thus composed of five theories; of these, four belong to class T, and one belongs to class $\bar{T}$. To determine the antecedent probability $P(A,T)$, we take the ratio of true theories in A to the total theories in A, i.e., 4/5. We have previously indicated that $P(A.T,E)$ is 1.0 in the case of a positive evidence report when the initial conditions of the theory held. $P(A,\bar{T})$ is the ratio of false theories to total theories in A, i.e., 1/5. $P(A.\bar{T},E)$ is then determined, by computing the ratio of the positive evidence reports obtained for a false theory (e.g., Theory five) to the total evidence reports for that theory; from the lattice it is seen to be 2/10 or 1/5. By substituting these values in Formula 1, the probability that the evidence report supports the theory can be seen to be:

$$P(A.E,T) = \frac{4/5 \cdot 1}{4/5 \cdot 1 + 1/5 \cdot 1/5} = .952.$$

If this value is considered sufficiently high (a judgment similar to that which the scientist must make in setting the significance levels for rejecting the null hypothesis), then it can be concluded that this is an instance of support of the theory.[8] If this probability is sufficiently low, the scientist may conclude that this is an instance of negation of the theory.

It should be added that Bayes's rule appears appropriate to the problem referred to above, where we have a number of different theories, all of which have the same consequence. The problem arises where we obtain a positive evidence report; we wish to determine for which one of the several theories that imply that consequence we have an instance of support. It would seem possible to determine the inverse probability for each of the theories, and then to select the one that best satisfies our significance levels. This application of Bayes's rule appears particularly knotty, but the general approach seems clear. The future worker should consider such problems as: (*a*) where the assumptions of mutually exclusive and exhaustive theories are not even approximately satisfied; (*b*) utilizing Bayes's postulate, a simpler form of Formula 1, when there is definite knowledge that the antecedent probabilities of the several theories are equal; (*c*) the need for developing separate reference classes (i.e., A) for each of the several theories, or possibly utilizing probability logic instead of a two-valued logic for stating our theories.

lattice is a prior requirement for determining an instance of support for a theory? By successive approximation on the basis of induction by enumeration, and particularly relying on an inequality developed by Reichenbach (**12**, p. 95), we should be able to answer this question with increasing precision. Further discussion of this point would lack the necessary brevity for presentation here.

[8] Reichenbach's criticism of inference by confirmation can now be made more explicit; i.e., obtaining a positive evidence report where the initial conditions of the hypothesis held supplies only the probability $P(A.T,E) = 1$, leaving unknown the other probabilities required by Bayes's rule.

CRITICISMS OF THE USE OF BAYES'S RULE

A number of criticisms of the general approach taken in this paper have been offered (e.g., **4, 7**). The essence of these criticisms seems to be that it is difficult to define a reference class, and that we do not have enough theories to provide us with the necessary mass of statistical data. These and similar criticisms are certainly important ones, and would compel us to abandon our attempts, *if we had available more effective and promising tools.* Even though the scientist many times is faced with problems for which his tools have apparent and glaring defects, he does not abandon his efforts but does his best with the tools he has. The differing approaches of other workers are promising, particularly that of Carnap (**3**).

The considerations here and elsewhere suggest that the establishing of probabilities of confirmation offers no difficulties in principle. That we do not have a great number of data relative to these problems, that the probabilities are difficult to interpret, and that the necessary reference classes are not easily defined, do not constitute objections to the procedures here outlined. As Reichenbach says,

> . . . speaking of the probability of an individual hypothesis offers no more logical difficulties than speaking of the probability of an individual event, say, the death of a certain person. . . . Should we some day reach a stage in which we have as many statistics on theories as we have today on cases of disease and subsequent death, we could select a reference class that satisfies the condition of homogeneity, and the choice of the reference class for the probability of theories would seem as natural. . . . In some domains we have actually been witnesses of such a development. For instance, we know how to find a reference class for the probability of good weather tomorrow; but before the evolution of a scientific meteorology this reference class seemed as ambiguous as that of a scientific theory may seem today (**12**, p. 440).

SUMMARY

The currently used procedure for testing theories, known as inference by confirmation, is invalid and is thus subject to error. The main difficulty with inference by confirmation is that a given theory does not uniquely imply a certain consequence; that consequence may also be implied by alternative theories of psychology. To make a valid inference (inference by indirect evidence), other probabilities must be taken into account, all of which are included in the rule of Bayes. In using Bayes's rule one must establish reference classes composed of homogeneous theories, and interpret the probabilities it contains. A concrete method of applying Bayes's rule is presented in the present paper, though a number of problems are unsolved. It is expected that eventually a complete methodology will evolve that will make a technique available to the experimentalist and to the theory maker, with which valid inferences can be made.

REFERENCES

1. BEHAN, R. A., & BEHAN, F. L. Comments on Seeman's operational analysis of the Freudian theory of daydreams. *Psychol. Bull.*, 1954, **51**, 176–178.
2. BERANDA, C. W. A five-fold skepticism in logical empiricism. *Phil. Sci.*, 1950, **17**, 123–132.
3. CARNAP, R. *Logical foundations of probability*. Chicago: Univer. of Chicago Press, 1950.
4. CHURCHMAN, C. W. *Theory of experimental inference*. New York: Macmillan, 1948.
5. FREUND, J. E. On the confirmation of scientific theories. *Phil. Sci.*, 1950, **17**, 87–94.
6. FREUND, J. E. On the problem of confirmation. *Methodos*, 1951, 33–42.
7. HELMER, O., & OPPENHEIM, P. A syntactical definition of probability and degree of confirmation. *J. symbol. Logic*, 1945, **10**, 25–60.
8. HEMPEL, C. G. Studies in the logic of confirmation. *Mind*, N. S., 1945, **54**, 1–26, 97–121.

9. JOHNSON, H. M. On verifying hypotheses by verifying their implicates. *Amer. J. Psychol.*, 1954, **67**, 723–727.
10. McGUIGAN, F. J. Formalization of psychological theory. *Psychol. Rev.*, 1953, **60**, 377–382.
11. MILLER, J. G. Symbolic technique in psychological theory. *Psychol. Rev.*, 1939, **46**, 464–479.
12. REICHENBACH, H. *The theory of probability*. Berkeley and Los Angeles: Univer. of California Press (2nd Ed.), 1949.
13. SALMON, W. C. The frequency interpretation and antecedent probabilities. *Phil. Stud.*, 1953, **4**, 44–48.
14. SEEMAN, W. The Freudian theory of daydreams: an operational analysis. *Psychol. Bull.*, 1931, **48**, 369–382.
15. SEEMAN, W. Reply to the Behans. *Psychol. Bull.*, 1954, **51**, 178–179.

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REINFORCEMENT AND EXPECTANCY: TWO THEORIES IN SEARCH OF A CONTROVERSY

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Behavior theories in America today are commonly grouped in three classes, following Spence (**17**): (*a*) *S-R* contiguity theories (Guthrie, Estes); (*b*) *S-R* reinforcement[1] theories (Hull, Spence, Miller, Thorndike); (*c*) *S-S* contiguity, or expectancy, or cognition theories (Tolman, Lewin). We are here concerned with the second and third of these categories. In view of recent developments it seems timely to ask what essential differences, if any, remain between them.

Chords and Discords

Both reinforcement and expectancy theories can trace their descent from the psychological hedonism of Hobbes, Bentham, and J. S. Mill. Cognition theory, for all its behavioristic claims, is held by its critics to have a trace of "mentalism" in its blood. A human, said the hedonist, acts with an eye to consequences, in such a way as to gain pleasure and avoid pain. So, in a sense, does Tolman's rat VTEing at a choice point. Reinforcement theory claims to have sprung from sturdy mechanistic stock; its forebears sought some simple automatic process to explain adaptive behavior without invoking foresight. Thorndike found it in the strengthening of *S-R* bonds by satisfying consequences. Hull found one such process in the increment of habit strength produced by drive reduction.

It is this attempt of the one theory to *derive* foresight, coupled with the other's willingness to depend on it, that has chiefly set the two schools apart. It also helps to explain their besetting weaknesses: *S-S* theory has always had trouble getting from sensory integration to response; *S-R* theory tends to be "response bound," to have trouble dealing with flexibility in a changing environment. On the whole each has proved a healthy corrective to the other, and their differences have stimulated a wealth of investigation on such problems as latent learning, place learning, and continuity in discrimination learning.

If these investigations have borne fruit, however, we should expect them either to substantiate one viewpoint at the expense of the other, or, more realistically, to produce such changes in both as eventually to reconcile their differences. MacCorquodale and Meehl's (**8**) recent attempt to formalize expectancy theory, coming soon after Hull's (**5**) last revision of his system, inevitably raises the question: how far have the two views moved toward each other, and on what issues are they still divided?

In a brief paper a point-by-point comparison of the two systems is out of the question. What we need is a single clear-cut issue most likely to reveal salient points of disagreement. Meehl and MacCorquodale (**10**) have anticipated our search. After rejecting several properties of expectancy theory as not clearly definitive, they selected as the most likely candidate for a "core concept" an *inference postulate*. Without claiming that cognitive theory alone could derive inferential behavior, they did hold that nonexpectancy doctrine would find it difficult.[2] Here, then, we

[1] *Reinforcement* is here used broadly to refer to the strengthening of an *S-R* connection by some other event occurring at the time.

[2] More precisely, they held that nonexpectancy theories could derive inference only by intro-

have a method of comparison. It consists of using the inference paradigm as a criterion of how much reinforcement and expectancy theories overlap. After briefly outlining the expectancy approach as formulated by MacCorquodale and Meehl (8), I shall try to show informally how inference has been and can be handled by reinforcement theory. This undertaking will enable us to consider pertinent developments in Hull's system, and to evaluate their significance for our general problem.

INFERENCE IN EXPECTANCY THEORY

Relevant Postulates

To be fully understood these postulates should be read in the context of the entire set. Here a much condensed statement, stripped of quantitative detail, must suffice. Such a statement is necessary partly to make the derivation intelligible, partly to show why the authors put so much stress on the inference postulate. The original numbers are retained.

1. *Mnemonization.* The occurrence of the sequence $S_1 \rightarrow R_1 \rightarrow S_2 \cdots$ results in an increment in the strength of an expectancy $(S_1R_1S_2)$ (8, p. 237).

4. *Inference.* The occurrence of a temporal contiguity S_2S^*, when $(S_1R_1S_2)$ has nonzero strength, produces an increment in the strength of a new expectancy $(S_1R_1S^*)$ (8, p. 242).

9. *Valence.* The valence of a stimulus S^* is a multiplicative function of the correlated *need* D and the *cathexis* C^* attached to S^* (8, p. 245).

12. *Activation.* The reaction potential ${}_sE_R$ of a response R_1 in the presence of S_1 is a multiplicative function of the strength of the expectancy $(S_1R_1S_2)$ and the valence . . . of the expectandum $[S_2]$ (8, p. 246).

Postulate 4 is significant because, in combination with the others, it provides a way of strengthening ${}_sE_R$ (aside from increasing D) without evoking R_1, a difficult feat for *S-R* theories. It does so by substituting a more strongly cathected expectandum. Two other methods we shall find useful to achieve a like result are given in the following additional postulates:

6. *Secondary cathexis.* The contiguity of S_2 and S^*, when S^* has valence $|V|$, produces an increment in the absolute cathexis of S_2 (8, p. 244).

7. *Induced elicitor cathexis.* The acquisition of valence by an expectandum S_2 belonging to an existing expectancy $(S_1R_1S_2)$ induces a cathexis in the elicitor S_1 (8, p. 244).

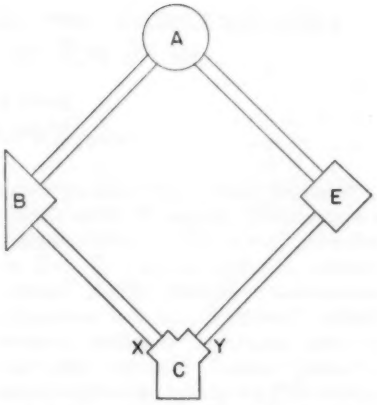


FIG. 1. Diagram of a hypothetical situation to demonstrate inference. End boxes A, B, C, and E are distinctive in shape and material. They are connected by enclosed runways. Access to all runways is controlled by sliding doors, of which X and Y are two.

An Experimental Paradigm

As an inference model take the situation depicted in Fig. 1. The experiment is conducted in three stages:

Stage I. A group of rats, 22 hours hungry, is given an equal number of forced trials in mixed order from C to B and from C to E, with no food in the apparatus and all other paths blocked.

ducing a mediating process such as the fractional antedating goal reaction r_G. It would be arbitrary to deny Hull the use of his own invention; our present interest is in how closely his use of it conforms to the tenets of expectancy theory.

Stage II. Half of the rats are run repeatedly from B to A where they find and eat food; the other half are similarly run from E to food in A. Again all paths are blocked other than the one used.

Stage III. All rats are placed hungry in C with doors X and Y open, and given a free choice. Inferential behavior is measured by the preponderance of choices of the path leading to the box (B or E) used in Stage II.

Positive results in comparable situations are not always obtained (**22**), but have been reported by several investigators (**9, 12, 21**).

Derivation

The following "proof" is based on the foregoing postulates, and modeled on the examples given by MacCorquodale and Meehl (**7**). In this and later derivations we need consider only the subgroup trained from B to A in Stage II.

1. As a result of I we have

$$(S_X R_X S_B) = (S_Y R_Y S_E) > 0. \qquad \text{By Postulate 1.}$$

2. In II the valence acquired by S_A from the food eaten in A induces a cathexis in S_B; i.e.,

$$C_{s_B} > C_{s_E}. \qquad \text{By Postulates 1, 6, 7.}$$
$$\therefore \; V_{s_B} > V_{s_E}. \qquad \text{By Postulate 9.}$$

3. In III we have

$$_{s_X}E_{R_X} = (S_X R_X S_B) \times V_{s_B}, \text{ and}$$
$$_{s_Y}E_{R_Y} = (S_Y R_Y S_E) \times V_{s_E}. \qquad \text{By Postulate 12.}$$
$$\therefore \; _{s_X}E_{R_X} > {}_{s_Y}E_{R_Y}. \qquad \text{By Steps 1 and 2.}$$

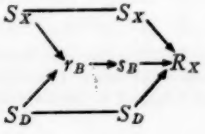
This argument leads directly to the greater probability of the correct response.

INFERENCE IN REINFORCEMENT THEORY

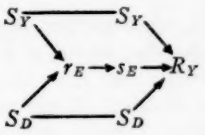
Early Derivations

Hull made two early efforts to account for inference; one (**2**) using a situation typical of Maier's (**9**) reasoning experiments, the other (**3**) modeled on Buxton's (**1**) latent learning experiment using a multiple-unit maze. His argument was essentially the same in both cases; applied to the present model it would run as follows:

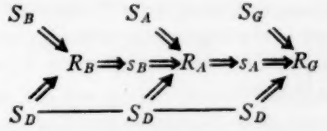
1. In I, with minimal reinforcement, a distinctive reaction to end box B is weakly conditioned to the drive stimulus (S_D) and generalized to the starting point (S_X), where its proprioceptive impulse (s_B) is conditioned to the approach response (R_X); i.e.,



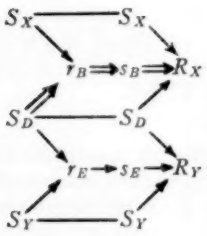
Similarly,



2. In II, through the powerful reinforcing agent (S_G) at A, the hunger drive stimulus is strongly conditioned to responses occurring in B; i.e.,



3. In III, at the choice point r_B is weakly aroused by S_X and strongly by S_D; r_B is weakly aroused by both S_Y and S_D; thus,



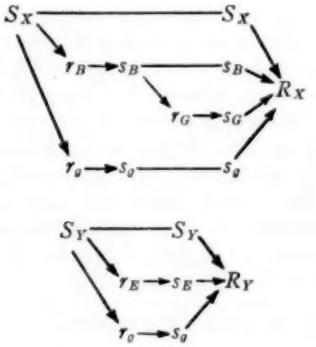
Since R_X is conditioned to stronger stimuli than is R_Y, it is more likely to occur. (Elsewhere [14] I have given reasons for doubting that the strength of a nonmotivating stimulus like s_B would determine choice.)

In his last book, Hull (5) returned to Maier's problem with a minor change of conditions and a new solution. His new model differed from ours in two ways: (*a*) In I he used a small food reward (S_g) in both end boxes; in II he used a large food reward (S_G). (*b*) He reversed the order of I and II; i.e., the rats were trained to the large reward in A *before* being trained to small rewards in B and E. His derivation was essentially as follows:[3]

1. In II the important result is the conditioning of r_G to stimuli from starting box B:

2. In I (which follows II), anticipatory response-produced stimuli, carried forward in the sequence by stimulus traces (and by S_D), are conditioned to R_X and R_Y as shown:

[3] The hypothetical role of S_D is here omitted to simplify the exposition. Since S_D was a constant factor throughout, it would contribute equally to both responses, aside from afferent interaction, leaving the balance of excitation otherwise unchanged.



3. In III we see again that more functional connections lead to R_X than to R_Y.

It is noteworthy that this time the extra bond comes from s_G, surrogate for the large reward, as it should from the standpoint of expectancy theory and common sense. Besides, the later analysis puts at our disposal another interpretation stemming from Hull's (4) 1943 principles: by association with the large reward, s_B has become a more powerful secondary reinforcer than s_E, thus making $s_X H_{R_X} > s_Y H_{R_Y}$.

But the new derivation is not entirely satisfactory. As Osgood (11, p. 618) has pointed out, it would work only if the large reward came first; otherwise s_G would have no chance to be conditioned to R_X. MacCorquodale and Meehl (8, p. 243), too, made it clear that we cannot escape this objection by invoking secondary reinforcement.[4] Unfortunately Hull's latest treatment evidently preceded his postulate revisions; had these been available he could have derived inference without restricting the order of the premises. Before attempting such a derivation we should therefore turn to the most recent form of his postulates.

Revised Postulates: 1952

Two major changes between 1943 (4) and 1952 (5) are worth noting because

[4] It should be noted that Hull's earlier derivation, based on conditioning of endbox reactions to S_D, was not subject to the same limitation.

their significance goes well beyond the deriving of inference. In my opinion they do much to remove any basic difference between Hull's position and Tolman's.

1. The first change was in the definition of habit strength ($_sH_R$). Formerly Hull had made this construct a function of a number of variables, including amount, delay, and number of occurrences of a reinforcing agent.[5] It was, indeed, this postulate that made Hull's a reinforcement theory. In 1952 he defined $_sH_R$ solely by number of reinforcements (Postulate IV, **5**, p. 6).[6] Amount of reward now determined a new function, *incentive motivation* (K), entering as a multiplier with drive (D), stimulus intensity dynamism (V), and $_sH_R$ to produce reaction potential ($_sE_R$); i.e., by Postulate VIII (**5**, p. 7),

$$_sE_R = D \times K \times V \times {}_sH_R. \quad [1]$$

The logical difficulty of separating the fact of a reward from its amount has already been pointed out (**6**, **13**). If a reinforcer must be present to produce an increase in $_sH_R$, it must be present to at least a threshold degree. Only above that threshold can the rate of habit increase be independent of variation in amount of reward. Hull's postulates imply that the least "diminution in the motivational stimulus" produces as much reinforcement as the greatest. But without a restrictive definition of "motivational stimulus" one can never deny that such a stimulus may have diminished slightly (cf. Koch, **6**, p. 106). In practice, then, if not in theory, Hull has reduced reinforcement to contiguity.

2. The second major change pertinent to our inquiry is in Hull's treatment of delay of reinforcement. Spence (**18**) earlier proposed a theory of delayed reward learning by immediate secondary reinforcement. This reinforcement resulted from two processes: (*a*) the conditioning of a consummatory response (R_G) to the stimulus pattern accompanying reward; (*b*) the arousal of a fractional consummatory response (r_G) by the stimulus pattern accompanying the correct response on the next trial. Strength of reinforcement depended on strength of r_G, which in turn depended on the stimulus generalization gradient assumed to extend over the period of delay. Elsewhere (**13**) I have pointed out that this theory differed from an expectancy formulation only in casting $r_G \rightarrow s_G$ as habit builder rather than as incentive.

In his final revision Hull (**5**) incorporated Spence's theory, and implicitly abandoned this distinguishing feature. To verify my statement, consider Corollary iii of Postulate VIII (**5**, p. 7). It includes two parts: (A) the effect of terminal reinforcement on the reaction potentials of successive responses in a behavior chain; (B) the effect of reinforcement at varying intervals after an isolated response. Part A, although it bears more directly on our inference paradigm, proved to be too complicated by overlapping traces for quantitative derivation. Part B, however, was worked out in enough detail to reveal its significance for the system as a whole. It is here reproduced as an equation:

$$J = {}_sE_{Rd} = D \times K \times V_S \times {}_sH_R \times 10^{-bd} \times V_{S'}, \quad [2]$$

where S is the distinctive stimulus pattern at the moment of R, S' is its trace at the moment of reinforcement, b is a constant, and $d = \log S - \log S'$ in units of stimulus intensity. We shall now consider its derivation.

[5] A reinforcer he defined as "the diminution of a need" (primary reinforcement) or "a stimulus which has been closely and consistently associated with the diminution of a need" (secondary reinforcement) (**4**, p. 178). Later he changed his definition of the primary reinforcer to "the rapid diminution in the motivational stimulus" (**5**, p. 6).

[6] In connection with stimulus intensity generalization, he added, with great uncertainty, a second factor, the intensity dynamism (V_1) of the original stimulus.

Suppose, borrowing Hull's example, that a rat is given distributed trials in a bar-pressing situation. When R occurs the bar is withdrawn; four seconds later a food reward (S_G) is presented and eaten (R_G). Stimulus pattern S is therefore produced partly by the bar, partly by R itself, and may be denoted by RS. Owing to the decline of stimulus-trace intensity (RS') during the delay, effective reaction potential between RS and r_G is given by the equation for stimulus intensity generalization (Postulate X B, **5**, p. 11):

$$_{RS}E_{r_G} = D \times K \times V_{RS} \times {}_{RS}\bar{H}_{r_G}, \quad [3]$$

where

$$_{RS}\bar{H}_{r_G} = {}_{RS'}H_{r_G} \times 10^{-bd} \times V_{RS'}. \quad [4]$$

To go from Equations 3 and 4 to Equation 2 we must assume that $_sH_R$ can be substituted for $_{RS'}H_{r_G}$ and treated in the same manner. But such a procedure implies that the $_sH_R$ in question actually involves a *residue* of R (R') conditioned at the moment of reinforcement to its own and other stimulus traces (RS'). To make this assumption is to explain delayed reward learning by falling back on the tenuous hypothesis of backward conditioning. Any such argument was surely far from Hull's intention.

We are forced to conclude that Corollary iii B is incorrectly derived, and that it does not properly refer to the instrumental response *as response* at all. The correct equation is No. 3, defining reaction potential between the stimulus of bar pressing and the anticipatory response to the food reward. It is quite possible that this is what Hull had in mind; i.e., to make J, or $_sE_{R_d}$, proportional to strength of goal anticipation. In that case Corollary iii B should be written:

$$_sE_{R_d} = c(_{RS}E_{r_G}), \quad [5]$$

where $_{RS}E_{r_G}$ is defined by Equations 3 and 4.

Before endorsing this definition let us consider the consequences:

1. The strength of an instrumental response is directly determined by the strength of the fractional goal stimulus (rs_G) aroused by it.

2. Both Spence and Hull held that rs_G produced immediate secondary reinforcement. If rs_G exerts its influence directly on $_sE_{R_d}$, and not by way of $_sH_R$, we must either abandon the reinforcement theory of delayed reward learning or restrict the term "reinforcement" to some meaning such as "facilitation."

3. As delay of reward approaches zero there is no reason to invent another mechanism of "reinforcement." Logically Postulate VIII should therefore be considered a special case of Corollary iii B where $d = 0$, making $_sE_R = c(_{RS}E_{r_G})$ and $_sH_R = {}_{RS}H_{r_G}$.

4. Hull contended that by means of the antedating reaction he had derived expectancy within his own system (**5**, pp. 108, 151 ff.). My interpretation of Corollary iii B, however, makes expectancy the keystone of the arch between the input and the output of behavior. Compare, for instance, MacCorquodale and Meehl's equation for $_sE_R$ with Hull's equation in the form here proposed:

MacCorquodale-Meehl:

$$_sE_R = D \times C \times S_1R_1S_2.$$

Hull ("corrected"):

$$_sE_R = c(D \times K \times V_{RS} \times {}_{RS}H_{r_G}).$$

Except for V, the status of which is doubtful (see **16**), and with due regard to underlying differences of definition, the parallel is obvious.

A Revised Derivation

With these revisions in mind let us return to our experimental paradigm and the derivation of inferential behavior. Reinterpreting Hull's postulates as I have suggested, we may now reason as follows:

1. In I, since all drive reduction cannot be ruled out, we assume that equal reaction tendencies are acquired between the stimuli of entering doors X and Y and the antedating reactions to end boxes B and E, respectively; i.e.,

$$_{RS}{}_{X}E_{r_B} = D \times K_B \times V_{RS_X} \times {}_{RS_X}\bar{H}_{r_B},$$

$$_{RS_Y}E_{r_E} = D \times K_E \times V_{RS_Y} \times {}_{RS_Y}\bar{H}_{r_E}. \quad \text{By Postulate X B and Corollary iii A.}$$

$$_{RS_X}E_{r_B} = {}_{RS_Y}E_{r_E} > {}_{S}L_R,$$

where ${}_{S}L_R$ is the reaction threshold.

2. As a result of II, stimuli produced in B arouse antedating reactions to the food received in A; i.e.,

$$_{RS_B}E_{r_G} = D \times K_G \times V_{RS_B} \times {}_{RS_B}\bar{H}_{r_G}. \quad \text{By Postulate X B and Corollary iii A.}$$

3. In III, on Trial 1, S_X produces an end-box surrogate rs_B (Step 1). But rs_B in turn arouses r_G (Step 2).

On the assumption that antedating reaction potentials can be summated, we therefore write:

(*a*)		${}_{S}{}_{X}E_{R_X} = c({}_{RS_X}E_{r_B} + {}_{RS_B}E_{r_G})$,	
(*b*)		${}_{S_Y}E_{R_Y} = c({}_{RS_Y}E_{r_E})$.	By Equation 5.
(*c*)	$\therefore$	${}_{S_X}E_{R_X} > {}_{S_Y}E_{R_Y}$.	By Steps 1, 3a, 3b.

It will be observed that the greater strength of ${}_{S_X}E_{R_X}$ is attributed to the goal-anticipatory component $({}_{RS_B}E_{r_G})$ indirectly aroused by S_X, *even though* rs_G *has never been associated with* R_X *itself.* As I tried to show in the preceding argument, this step is justified because Hull made ${}_{S}H_R$ depend solely on *number* of reinforcements, and held delay of reward, as represented by r_G, to determine performance rather than habit formation. With or without his intent these revisions strongly imply a nonassociative, activating, or motivating role for r_G. If my interpretation is valid, Hull has avoided the criticism of Osgood and of MacCorquodale and Meehl, as previously mentioned, but only by abandoning the associative function of reward.

CLOSE HARMONY

A few years ago Spence wrote that, in his opinion, "as Tolman's theory is developed, the essential similarity of its formal structure to that of Hull's theory will become more and more apparent" (**19**, p. 706). Since it remained for two admitted "fifth columnists" (**7**, p. 63) to "formalize" Tolman's theory, such an outcome would not be too surprising. More surprisingly, it appears that Spence would have predicted better if he had interchanged Tolman with Hull in the above quotation. Two examples from the preceding section will suffice: (*a*) Spence went on to say: "The most fundamental difference between them at the moment is the question whether or not reinforcement is a necessary condition for . . . learning." We have seen that in Hull's present system the reinforcer directly determines performance; its role in learning is reduced to a pseudoproblem. (*b*) MacCorquodale and Meehl held that a distinguishing feature of their formalization was the ease of deriving inference. We have seen that Hull's present system need have no greater difficulty with this problem, since it provides a closely analogous solution.

In bringing out points of agreement between the two theories I have probably undervalued their differences. From a heuristic standpoint this is a more serious error than its opposite. As long as two theories in good standing lead to measurably different predictions, both have a function, regardless of their ultimate

truth value. What remaining issues entitle reinforcement and expectancy to coexistence?

1. First to come to mind is the *S-R* versus *S-S* dichotomy, the question whether learning connects afferent impulses exclusively with efferent ones or also with other afferents. The importance of the issue is debatable: MacCorquodale and Meehl (**8**, **10**) have shown as much by debating it; Tolman (**21**), by disclaiming interest in it; and Spence (**20**), by pointing out that Hull's mathematical definition would apply equally well to ${}_{S}H_{R}$ and ${}_{S}H_{S}$. My own use of an ambiguous construct, the surrogate response (*rs*), as a mediating process, simply sidesteps the problem (**15**). The reason for this neglect is that the question, as its wording implies, refers to physiological, not behavioral, events. At the behavioral level the important thing is that both theories have a mechanism by which anticipated events can modify present behavior. Whether that mechanism is sensory or motor can be decided only by special techniques such as neurosurgery or implanted electrodes. The existence of such techniques, however, gives the distinction meaning; its importance (whatever that may mean) can hardly be prejudged. Meanwhile an *S-R* position can be defended insofar as it alerts us to the possibility of eavesdropping on expectancy, whether by measuring VTE, action currents, galvanic skin responses, or salivary secretion.

2. The next point concerns no specific issue but something more subtle. It has to do with the common-sense distinction between explanation and prediction. Two theories may both prove capable of deriving, or explaining, an observed outcome, but only one of them may be likely to suggest it. The fruitfulness of a theory may, indeed, bear little relation to its structural formality. In its early stages every system generates experimental predictions, not out of clearly stated postulates, but from loosely phrased assumptions about the nature of its subject matter. One danger of premature formalization is that this period of relatively free exploration will be cut short.[7] A cognition theorist, e.g., thinks of response as incidental to perception. Taking high-level, integrated behavior as the norm, he looks for evidence of flexibility, insight, reasoning—and is frequently disappointed when simple organisms fail to provide it. A reinforcement theorist, on the other hand, thinks of a response as reflecting the strength of its connection to some immediate stimulus (${}_{S}E_{R}$). He seeks to describe the conditions determining ${}_{S}E_{R}$ as a set of continuous functions—and is disturbed by the discontinuities that sometimes accompany a change in external or internal conditions. *After* this happens he can often account for it by introducing a functional equivalent of the mediating process that the cognition theorist started out with. My point is that each doctrine has an underlying bias that encourages a different type of speculation. The leavening effect of these attitudes on research is worth preserving as long as possible.

3. As theories crystallize, deductions must inevitably conform more closely to the specific form of relevant postulates. Loss of flexibility is balanced by the sharpening of discrepancies between prediction and outcome. Gross parallels between tentative postulate sets, such as those we have noted between MacCorquodale and Meehl's and Hull's, should not blind us to the finer quantitative differences that will continue to stimulate and guide research.

We have drawn a picture of two approaches converging toward a common set of principles. Major controversial

[7] It may be no accident that so many of the controversies between adherents of Tolman and Hull—e.g., latent learning, irrelevant incentive learning, "hypotheses" in rats, place learning—started with experiments either suggested or carried out by Tolmanites.

issues will undoubtedly become harder to find. It may console belligerents on both sides to remember that the primary function of a systematic viewpoint is not to win battles but to make testable predictions.

SUMMARY

This paper is an attempt to appraise the present status of reinforcement and expectancy theories as exemplified by Hull's (**5**) latest revision and MacCorquodale and Meehl's (**8**) recent "formalization" of Tolman's view. Specifically, the two systems are compared in their approaches to the derivation of inferential behavior. Hull's redefinition of habit strength changes the meaning of "reinforcement," while his use of r_G in deriving the effect of delayed reward makes goal anticipation the major determinant of reaction potential. As a result of these changes, inference can be derived without the restrictions met by Hull's earlier attempts, in a manner closely analogous to that of expectancy theory.

REFERENCES

1. BUXTON, C. E. Latent learning and the goal gradient hypothesis. *Contr. psychol. Theory*, 1940, **2**, No. 6.
2. HULL, C. L. The mechanism of the assembly of behavior segments in novel combinations suitable for problem solution. *Psychol. Rev.*, 1935, **42**, 219–245.
3. HULL, C. L. Memoranda to Psychology 106. In *Psychological memoranda, 1940–1944*. Bound mimeographed material on file in the libraries of the University of Iowa, University of North Carolina, and Yale University. Nov. 14, 1941.
4. HULL, C. L. *Principles of behavior*. New York: Appleton-Century, 1943.
5. HULL, C. L. *A behavior system*. New Haven: Yale Univer. Press, 1952.
6. KOCH, S. Clark L. Hull. In W. K. Estes, S. Koch, K. MacCorquodale, P. E. Meehl, C. G. Mueller, Jr., W. N. Schoenfeld, & W. S. Verplanck. *Modern learning theory*. New York: Appleton-Century-Crofts, 1954. Pp. 1–176.
7. MACCORQUODALE, K., & MEEHL, P. E. Preliminary suggestions as to a formalization of expectancy theory. *Psychol. Rev.*, 1953, **60**, 55–63.
8. MACCORQUODALE, K., & MEEHL, P. E. Edward C. Tolman. In W. K. Estes, S. Koch, K. MacCorquodale, P. E. Meehl, C. G. Mueller, Jr., W. N. Schoenfeld, & W. S. Verplanck. *Modern learning theory*. New York: Appleton-Century-Crofts, 1954. Pp. 177–266.
9. MAIER, N. R. F. Reasoning in white rats. *Comp. Psychol. Monogr.*, 1929, **6**, No. 3.
10. MEEHL, P. E., & MACCORQUODALE, K. Some methodological comments concerning expectancy theory. *Psychol. Rev.*, 1951, **58**, 230–233.
11. OSGOOD, C. E. *Method and theory in experimental psychology*. New York: Oxford Univer. Press, 1953.
12. SEWARD, J. P. An experimental analysis of latent learning. *J. exp. Psychol.*, 1949, **39**, 177–186.
13. SEWARD, J. P. Secondary reinforcement as tertiary motivation: a revision of Hull's revision. *Psychol. Rev.*, 1950, **57**, 362–374.
14. SEWARD, J. P. Experimental evidence for the motivating function of reward. *Psychol. Bull.*, 1951, **48**, 130–149.
15. SEWARD, J. P. How are motives learned? *Psychol. Rev.*, 1953, **60**, 99–110.
16. SEWARD, J. P. Hull's system of behavior: an evaluation. *Psychol. Rev.*, 1954, **61**, 145–159.
17. SPENCE, K. W. Theoretical interpretations of learning. In F. A. Moss (Ed.), *Comparative psychology* (Rev. Ed.). New York: Prentice-Hall, 1942. Pp. 280–329.
18. SPENCE, K. W. The role of secondary reinforcement in delayed reward learning. *Psychol. Rev.*, 1947, **54**, 1–8.
19. SPENCE, K. W. Theoretical interpretations of learning. In S. S. Stevens (Ed.), *Handbook of experimental psychology*. New York: Wiley, 1951. Pp. 690–729.
20. SPENCE, K. W. Theoretical interpretations of learning. In C. P. Stone (Ed.), *Comparative psychology* (3rd Ed.). New York: Prentice-Hall, 1951. Pp. 239–291.
21. TOLMAN, E. C., & GLEITMAN, H. Studies in learning and motivation. I. Equal reinforcements in both end-boxes, followed by shock in one end-box. *J. exp. Psychol.*, 1949, **39**, 810–819.
22. WOLFE, J. B., & SPRAGG, S. D. S. Some experimental tests of 'reasoning' in white rats. *J. comp. Psychol.*, 1934, **18**, 455–469.

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TWO-FACTOR LEARNING THEORY RECONSIDERED, WITH SPECIAL REFERENCE TO SECONDARY REINFORCEMENT AND THE CONCEPT OF HABIT [1]

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By 1947 no less than ten writers had come to the conclusion, more or less independently, that there are two separate and distinct learning processes. In that year the present writer (**24**) reviewed the evidence for and against this position, and dubbed it "two-factor" theory. And a more recent review of the literature (**25**) shows that since that date a good many others have adopted, with minor variations, the same point of view.

Basically the two-factor, or two-process, hypothesis holds that *habits* are learned on the basis of the reinforcement provided by reward, or drive reduction, and that *fears* are learned (conditioned) on the basis of contiguous occurrence of a signal and punishment, in the sense of drive induction. Pavlov had held that *all* learning is a matter of conditioning or stimulus contiguity, while Thorndike and Hull had stressed habit formation on the basis of reward. Two-factor theorists, by contrast, have held that it is not here a matter of either-or but of *both:* both sign learning (conditioning) and solution learning (habit formation).

It will not be necessary at this time to review the various arguments favoring the two-factor position. Instead, attention is directed to two palpable weaknesses. In the first place, the concept of habit, as a *bond* or *connection* involving lowered synaptic resistance between a given stimulus or situation and a fixed, stereotyped response, has continued to draw—from Lewin, Tolman, Lashley, and many others—a variety of cogent criticisms. Moreover, as a result of extensive laboratory work on what has been designated as secondary reinforcement, we know that fear conditioning is not the only kind of sign learning; with reinforcement of the type which is supposed to be peculiarly appropriate for habit formation, a type of conditioning also occurs.

In the pages which follow, an attempt will be made to reformulate two-factor theory so as to accommodate the well-established fact of secondary reinforcement. And such a reformulation leads, we shall discover, to a considerably modified conception of habit, one which is basically compatible, as the traditional conception has not been, with "field theory" and which has various other integrative possibilities.

Punishment and Avoidance Learning

For a long time psychologists were inclined to assume, with Thorndike, that punishment was merely the antithesis of reward, reward "stamping-in" habits and punishment "stamping-out" such bonds or connections. Learning was thus seen as a reversible process, reward causing it to move in one direction and punishment causing it, simply and directly, to be reversed, obliterated.

But then, roughly twenty years ago, a number of workers became interested

[1] Read at the October, 1954, meeting of the Illinois Psychological Association, and at DePauw University in May, 1955. For highly useful criticisms of a preliminary draft of this paper (see last section), the author is indebted to M. E. Bitterman, F. S. Keller, K. S. Lashley, K. F. Muenzinger, Murray Miron, J. P. Seward, and R. L. Solomon.

in what has since been termed avoidance learning. Here a painful, "punishing" stimulus was used, not as a means of inhibiting an already established response, but as a means of instituting a new one. For example, Hunter (13) constructed a circular alley, the floor of which consisted of several metal grills which could be independently electrified. He then found that if a rat was placed in such an apparatus and if a buzzer was briefly sounded before the particular section of grill on which the rat was sitting was electrified, only a few repetitions of this procedure were required before the rat, upon presentation of the buzzer, would run to the neighboring section of the floor grill, thus *avoiding* the shock which would have been applied if the rat had remained where it was when the buzzer came on.

Some investigators were inclined to interpret avoidance learning of the kind just described in terms of simple conditioning. Running was said to be the unconditioned response to shock; and after buzzer and shock had been paired a few times, it was only to be expected, according to this view, that the running response would start occurring, anticipatorily, to the buzzer alone. But further analysis of this phenomenon has shown that it cannot be interpreted so simply. It is now clear that the first thing that happens in a situation of the kind described is that the buzzer acquires the capacity to make the subject *afraid;* then, as a seemingly quite separate process, the subject learns to *do* whatever will reduce its fear. This may involve behavior which is quite unlike that manifested in response to the unconditioned stimulus (28).

Once this way of thinking—which, incidentally, played an important role in prompting the original two-factor distinction between sign learning and solution learning—was well established, it was but a short step to a new theory of punishment (22). If an *external* stimulus, like the buzzer in the Hunter experiment, becomes conditioned to the emotion of fear as a result of being paired with shock, then does it not follow that an *internal* stimulus or pattern of stimuli, produced by an organism's own behavior, will likewise, if paired with shock, become conditioned to fear? But in this case, though the principle be the same, the effect is different. In the Hunter-type situation the animal reduces fear and avoids shock by *doing something*, whereas in the situation just described the best way of reducing fear and avoiding shock is to *do nothing*, or at least nothing very similar to the action which has been associated with punishment.

In this way, by using the same set of principles, we account equally well for situations where "punishment" is used to *instigate* behavior and where it is used to *inhibit it*. These principles now seem to be generally accepted (4, 7, 37, 42).

FEAR CONDITIONING AND SECONDARY REINFORCEMENT

In the preceding discussion, the central concept is fear conditioning. As is well known, this is best achieved by pairing a formerly neutral stimulus with the onset of a painfully intense stimulus. The present writer has previously suggested (23) that fear might be thought of as the *conditioned form* of the pain reaction; but Miller (20) has made a different and probably more nearly correct suggestion. He has pointed out that intense, noxious stimulation produces not only the sensation of pain but also directly arouses fear and that, in conditioning, this response simply gets transferred from the noxious stimulus to an originally neutral one. In other words, Miller suggests that fear is an inherent component of the total reaction, or drive state, produced by painfully intense stimulation but

that this component can be *detached* from its original constellation and made to occur in, so to say, pure form by the process of conditioning. This interpretation of fear conditioning is schematized in Fig. 1.

In the preceding section it is assumed that once a fear has become connected to a particular stimulus or situation, any response which lessens the fear, i.e., eliminates the stimulus causing it, will be reinforced. If the fear-arousing stimulus is external to the organism, behavior will be reinforced if it carries the organism away from the stimulus; and if the fear-arousing stimulus is internal, i.e., response-produced, the organism will be relieved, rewarded, reinforced for discontinuing the behavior which is responsible for such stimulation. In the one case, the organism may be said to be motivated to "get out of harm's way"; in the other case, to "avoid trouble." In both instances, however, the resulting behavior is fear-prompted and is reinforced by fear reduction.

If behavior is motivated by a biologically given, or "primary," drive such as hunger or thirst, the relief and satisfaction experienced when the drive is reduced are known as *primary reinforcement*. It was therefore quite natural that as our understanding of the motivational and reinforcing properties of fear increased, there would be a tendency to speak of this emotion as an acquired, or "secondary," drive and of the relief and satisfaction experienced when it is reduced as *secondary reinforcement*. This usage has numerous advocates (22, 29a, 37), but it is at variance with another interpretation of the same expression.

Long ago Pavlov (31) demonstrated what he called second-order, or secondary, salivary conditioning; and, in 1943, Hull (12) reviewed a wide range of evidence showing that a stimulus which has been associated with food (hunger reduction) not only has the capacity, which Pavlov had noted, to mediate higher-order conditioning, it can also be used to establish new, instrumental acts, or "habits," just as can food itself. This latter capacity, which is acquired by a stimulus that is associated not only with hunger reduction but with the reduction of *any* primary drive, has also, reasonably enough, come to be known as *secondary reinforcement*, despite the fact that it seems, superficially at least, to be something quite different from the type of secondary reinforcement first discussed.

Fortunately, the Miller interpretation of fear conditioning points the way to a possible reconciliation of these two seemingly discordant views of secondary reinforcement. If fear, as shown in Fig. 1, is part of the total response produced by an intense primary drive, then fear reduction ought to occur shortly after termination of the primary drive, as suggested schematically in Fig. 2. If, moreover, a signal occurs just before the primary drive terminates, one

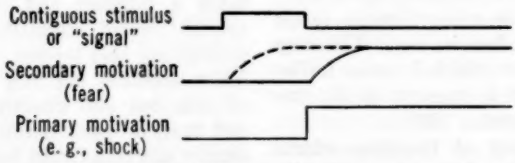

FIG. 1. Diagrammatic representation of fear conditioning. This emotion is originally elicited by some primary drive, such as electric shock; but if some other, formerly neutral, stimulus immediately precedes the primary drive, the fear response, after a few pairings of signal and drive, will become attached to the signal and "move forward," as shown by the broken line. When this occurs, the fear response is said to have become anticipatory; it is then capable of independently motivating and reinforcing behavior.

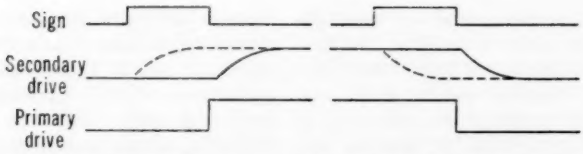

FIG. 2. An extension of the thinking represented in Fig. 1. Here it is assumed that an intense primary drive, such as electric shock, will be accompanied by some degree of apprehension or fear throughout its duration, and that this fear will terminate shortly after the primary drive terminates. If, now, a signal regularly and closely precedes these two events, it is not unlikely that it will become capable of producing a lessening of fear, or arousal of "hope," somewhat in advance of termination of the primary drive, as shown by the broken line. This type of conditioning is here assumed to provide the basis of one type of secondary reinforcement.

might suppose that the fear reduction would become anticipatory and occur, in response to the signal, as shown in Fig. 2 by the broken line. Many commonplace as well as laboratory observations give credence to this inference. One example, taken from the current *Reader's Digest*, will suffice. In an article entitled, "The Why of Aches and Pains," Edward and Ruth Brecher (3) describe some recent researches by Hardy, Wolff, and Goddell on pain and pain alleviators. Here they say:

> Mental expectation of pain relief has much the same effect as chemical pain-relievers such as aspirin. Drs. Hardy and Wolff have compared the effect of aspirin with the effect of fake pills which the patient thinks are aspirin. They report that about half of the pain-relieving effect is produced by the aspirin itself and the other half by the patient's confident expectation that the tablet will relieve him (3, pp. 75–76).[2]

But how does all this bear upon the Pavlov-Hull type of secondary reinforcement? It is well known that a stimulus which has been associated with hunger or thirst reduction works as a secondary reinforcer best (in many instances only) if the drive with whose termination it has been associated is again present. In other words, the secondary reinforcing stimulus seems to give promise of relief from the "pain" caused by the primary drive, i.e., it reduces the fear that accompanies any strong, persistent drive. If such a drive is not present, then a stimulus promising relief therefrom is not ordinarily very meaningful or effective.

In this relatively simple way, one can harmonize the two interpretations of secondary reinforcement mentioned above. Both, it seems, involve a decrement in the secondary drive of fear. But there is this not insignificant difference: in the one case, the fear is reduced by disappearance of the stimulus or signal which, alone, has aroused the fear; whereas, in the other case, the fear is reduced by the appearance of a stimulus which promises (gives "hope") that the primary drive which is causing the fear will shortly terminate. Hence we are led to speak of *two types* of "fear conditioning": conditioned *arousal* of fear and conditioned *relaxation*, or relief, thereof. Secondary reinforcement, decremental type, may thus be mediated either by the end of a stimulus which has been associated with the onset of a more primary discomfort *or* by the onset of a stimulus which has been associated with the end of such discomfort.

TWO-FACTOR THEORY RECONSIDERED AND REVISED

Two-factor learning theory, in its original version, held that "habits," i.e., instrumental, behavioral solutions to problems, are learned on the basis of drive reduction and that fear becomes

[2] For a more technical presentation of the evidence for this notion, see Elithorn, Piercy, and Crossley (7a).

conditioned to new signals or signs on the basis of drive induction. In the preceding section it has been shown that there are, in reality, two types of fear conditioning, namely, that whereby a formerly neutral stimulus becomes capable of *arousing* fear and that whereby a formerly neutral stimulus becames capable of *allaying* fear—and producing "hope." By recognizing these two forms of fear conditioning, rather than only one, it is possible, as shown, to reconcile and integrate two seemingly incompatible conceptions of secondary reinforcement. This reconciliation, we then discover, has important implications for the concept of habit, and leads to a revision of two-factor theory.

Incremental fear conditioning and its uses are relatively well understood. We know, as previously indicated, that if fear arousal is conditioned to a stimulus which is external to the organism, the organism will be motivated by the fear to get rid of that stimulus, and very often the best way of doing that is to retreat from it. If, on the other hand, fear arousal becomes conditioned to a stimulus that is internal (response-produced), the organism will be motivated, no less than in the other type of situation, to get rid of the fear; but now the best way of doing so is for the organism to inhibit the response which produces the fear-arousing stimulation.

The question which naturally suggests itself next is this: Are there, in the case of *decremental* fear conditioning, two different types of effects which likewise depend upon whether the conditioned stimulus is internal or external to the organism? We now know something about what happens if an *external* stimulus acquires the capacity to reduce fear, or arouse hope. This is the familiar phenomenon of secondary reinforcement, in the sense in which Pavlov and Hull used the term. Let us take an example. Suppose that a tone has been sounded for a few seconds just before a hungry rat is given food. After several repetitions of this tone-food sequence, it will be found that the rat is apparently *relieved by* and "likes to hear" the tone. This can be demonstrated by simply making it possible for the rat to produce the tone by pressing a little metal bar (35). The rat will press the bar repeatedly, until the tone, because it is not now followed by food, eventually loses its reassuring, hope-inducing capacity. The rat, so to say, "hoards" the tone, presumably because it feels "better," "safer," when hearing than when not hearing it. Secondary reinforcement, as thus mediated by an external stimulus, can be demonstrated in numerous other ways; but this one example will suffice for present purposes.

Now against the background of the foregoing discussion, one will wish to know what happens if secondary reinforcement, or fear decrement, becomes conditioned to *internal, response-produced* stimuli. A commonplace illustration is instructive.

Suppose one takes the right foreleg of a gentle, friendly, but somewhat hungry dog and flexes it acutely—so as to produce a distinctive pattern of internal stimulation—and then gives the dog a bit of food. In this way, hope, reassurance, fear relaxation, or secondary reinforcement becomes conditioned to a specific set of proprioceptive and other stimuli. What is the result? Since the dog can itself produce such a pattern of stimulation (like the rat when the bar is made available), it will not be long until we see the dog "voluntarily" flexing its right foreleg. We then say the dog is offering to "shake hands" or that it is "begging" for food. But whatever the *social* implications of this action, we can confidently infer that the dog "likes the feel" of the pattern of self-stimulation thus produced, just as the rat, in the preceding illustration, appears to enjoy hearing the external tone.

Suppose we then use a different procedure. Instead of ourselves producing in the dog the particular pattern of internal stimulation which we wish to reinforce, let us wait until the dog, in the course of its own spontaneous behavior, makes the required response. Reward is administered whenever this response occurs, and gradually its frequence of occurrence increases. Here the procedure may be somewhat less efficient than where the dog is "put through" the behavior which produces the desired pattern of stimulation; but there seems to be no difference in principle. In both cases, what we have presumably done is to attach secondary reinforcement to a particular set or pattern of stimuli; and it does not matter much whether the stimulus is external or internal: the organism will in both instances try to "get more" of the stimulus and in so doing will behave in a specifiable and distinctive way.

Under the influence of Thorndike, our tendency has been to think of habit formation as follows. An organism is prompted, by some drive such as hunger, to engage in trial and error, or "variable," behavior. One of the resulting responses, which in the beginning is but weakly connected with this drive, proves to be correct, i.e., gets rewarded. The result: a strengthening of the neural "bond" between the drive and this particular response. This is the essence of habit formation, as traditionally—and still widely—conceived.

In the present paper we are trying to look at the learning involved in so-called habit formation in a different way. Here we are thinking of it, not as the strengthening of a neural connection or pathway between a drive and a particular piece of behavior but rather as the strengthening of the neural pathway *between the stimuli produced by a particular response and the phenomenon of secondary reinforcement.*

Twenty years ago, punishment and avoidance learning seemed to be quite different orders of events. Today we explain them both in terms of a common, simple set of principles. Until recently we have tended to think of habit and of secondary reinforcement as distinct phenomena. Secondary reinforcement could strengthen habit, but they were different. Here we are exploring the possibility that both habit and secondary reinforcement are, at bottom, the same sort of thing; both, it seems, involve the conditioning of fear decrement, or "hope," to particular stimuli. Whether these stimuli originate within the organism or are external to it is of practical but apparently no theoretical importance.[3]

SUPPORTING LABORATORY EVIDENCE

A well-established laboratory finding will help concretize this revised notion of habit. Originally Bugelski (5) and, more recently, Miles [4] have reported the following sort of observation. Suppose the habit of bar-pressing is established in rats as a means of obtaining food. The "strength" of such a habit is often measured in terms of its resistance to extinction, i.e., its persistence after the withdrawal of food as a reward. In this connection it has been found that resistance to extinction is greater if performance of the response is accompanied by one or more external secondary reinforcers. For example, suppose a buzzer has sounded each time a rat has pressed the bar and obtained a pellet of food. The finding is that bar-pressing will continue longer if, after food has been withdrawn, the buzzer continues to oc-

[3] Pieces of this argument have been developed in earlier studies (26, 27; see also the writer's "Is 'habit strength' merely 'secondary reinforcement'?", mimeographed, 1953, pp. 76). It is hoped that book-length discussion and relevant research will follow.

[4] Cf. Miles, R. C. The relative effectiveness of secondary reinforcers throughout deprivation and habit-strength parameters (to be published).

cur each time the bar is pressed than it will if the buzzer is also eliminated.

The conventional interpretation of this finding has been to the effect that, after primary reinforcement (food) is discontinued, habit strength is kept up by the secondary reinforcement provided by the buzzer. The alternative suggested here is that secondary reinforcement *is* the habit and that if *all* stimuli which have been associated with hunger reduction were eliminated, the habit would disappear forthwith. Normally, there are both external and internal stimuli which accompany the occurrence of a rewarded, or punished, response. The external stimuli can often be eliminated, as in the type of experiment just cited, with an ensuing loss of habit strength. The assumption is that if the associated *internal* stimuli, arising from the movement of muscles, tendons, and joints and from cutaneous and pressure receptors could likewise be eliminated, the loss of habit strength would be immediate and total.

Although there is apparently as yet no empirical support for the assumption, it follows from the foregoing analysis that the effects produced by punishment could be likewise manipulated. If a buzzer accompanied some punished act, recovery from the resulting inhibition would probably be more swift if the buzzer were disconnected; and if the internal stimuli associated with the act could be eliminated, one would expect the punished, and thereby inhibited, response to "come back" very rapidly indeed and perhaps quite completely.

Experimental work carried out by Loucks (17) and by Konorski (15, 16) provide other illustrations of the general applicability of the hypothesis advanced in this section.

HABIT, FEEDBACK, AND FIELD THEORY

It would be misleading if the conception of habit just described were presented as something entirely new. The present writer hopes he does no injustice to the thinking of field theorists in general and that of Tolman (38, 39, 40) in particular in suggesting that the present formulation has a good deal in common therewith.

The principal difference lies, perhaps, in this: Although field theory and the present analysis arrive, in the end, at a somewhat similar position, they reach this point by different routes and from different starting points. The present discussion has done little more than trace the evolution of stimulus-response psychology over the past quarter of a century. If, in psychology, we are beginning to see that "all roads lead to Rome," this will hardly be an occasion for regret.

Added justification for such optimism is to be found in recent publications by Birch and Bitterman (2), Denny and Adelman,[5] Dinsmoor (6), McClelland (18, 19), Muenzinger (29), J. P. Seward (32, 33, 34), and in the two publications by Konorski (15, 16) already cited. It is not possible here to make a detailed comparison of the theoretical positions of these writers and that suggested in the present paper. At first blush, the differences may seem more striking than the similarities; but careful study of the works cited will show that they are focusing upon many of the same problems and that, in many instances, they arrive at fundamentally similar conclusions.

Movement away from the S-R bond conception of habit has undoubtedly been facilitated in the minds of various psychologists by recent developments in the field of cybernetics (36, 41). Here the emphasis falls, not upon machines which merely *transmit power*, by means of pulleys and gears and drive rods, but

[5] Cf. Denny, M. R., & Adelman, H. M. Elicitation theory II: The formal theory and its application to instrumental escape and avoidance conditioning. Unpublished manuscript.

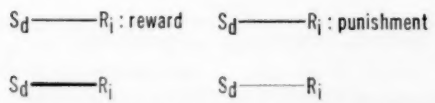

FIG. 3. Schematic representation of the two halves, or aspects, of Thorndike's Law of Effect. It held, in essence, that if some drive stimulus S_d produces some instrumental (behavioral) response R_i, and if R_i is then followed by reward, this S_d——R_i sequence, or *bond*, will as a result be strengthened (diagram at left) and that if such a stimulus-response sequence is followed by punishment, the bond will as a result be weakened (diagram at right).

upon machines into which are built delicate devices for *self-regulation*. Here the phenomenon of *feedback* is crucial: the machine is so constructed that it responds to the effects produced by *its own behavior* and in this way achieves a level of "intelligence" far transcending that exhibited by any device which is a mere power conveyor.

If learning is the central factor in the intelligence of living organisms, would it seem more likely to involve changes in the neural (motor) pathways which transmit power or in the neural pathways which connect external and internal sources of information with the "central" controlling mechanisms? In the present paper it is suggested that all learning is in the nature of sign learning, significance learning, meaning learning. When a stimulus (external of internal) is repeatedly paired, or correlated, with a bad experience, i.e., with drive · increment, a meaning (fear) is learned which tends to prevent recurrence of that experience. On the other hand, when a stimulus (external or internal) is repeatedly paired with a *good* experience, i.e., with drive decrement, a meaning (fear decrement or hope) is learned which tends to insure recurrence of that experience. Thus, habit formation and fear conditioning (incremental type), which were the two factors in the original version of two-factor learning theory, give way to incremental fear conditioning or secondary motivation and *decremental* fear conditioning or secondary reinforcement. These phenomena correspond, respectively, to the negative and positive valences, vectors, or expectancies of field theory and, at least loosely, to the two kinds of "information"—"on" and "off," "yes" and "no"—involved in servo (feedback) mechanisms.

ORIGINAL AND REVISED TWO-FACTOR THEORY COMPARED

The original version of two-factor learning theory now appears, in retrospect, as a sort of stepping stone, intermediate between monistic interpretations and the revised two-factor position here presented. The systematic relationships here involved may be summarized as follows.

Thorndike's Law of Effect is schematized in Fig. 3 and the Pavlovian conception of conditioning is represented in Fig. 4. Each alone, according to its exponents, was capable of accounting for the basic facts of learning; but others felt that both conceptions were necessary. Two-factor learning theory, as envisioned a decade ago, accepted, essentially unmodified, the "first half" of the Law of Effect, which held that reward strengthens habit; but it departed from the Law of Effect in holding that

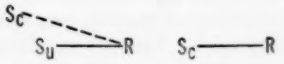

FIG. 4. Schematic representation of conditioning, as conceived by Pavlov. S_c is a stimulus which, initially, is not capable of eliciting response R, but which, as a result of contiguous occurrence with S_u (above, left), acquires this capacity (above, right). The subscripts c and u are used to designate, respectively, what Pavlov termed the "conditioned" and the "unconditioned" stimulus.

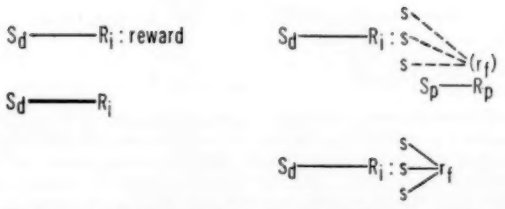

FIG. 5. Schematic representation of the original version of two-factor learning theory. According to this formulation, habits were made by reward, as shown at the left, in essentially the way suggested by the Law of Effect. However, punishment was assumed to involve fear conditioning, as shown at right, with ensuing conflict and probable inhibition (see text).

punishment achieves its action, not by simply reversing the effects of reward, but by causing fears to become attached (conditioned) to stimulation associated with the occurrence of the punished response. This version of two-factor theory is shown in Fig. 5. Here habit formation, or solution learning, is conceived essentially as Thorndike suggested; but punishment is seen in more complex terms. Instead of simply reversing the effect produced by past reward, punishment is here thought of as providing the basis for the conditioning of fear, which then produces conflict and, if the fear is strong enough, inhibition.

The details of this interpretation of punishment are shown at the right in Fig. 5. Here a problem situation or drive, S_d, produces an overt, instrumental response, R_i. R_i is followed by punishment, which is here represented as S_p. The punishment elicits a response R_p, of which r_f, or fear, is a component. But when R_i occurs, it not only produces the extrinsic punishment, S_p; it also produces a number of other stimuli, s, s, s, which are inherently related to the occurrence of R_i. The result: a part of the reaction produced by S_p, namely fear, gets conditioned to these response-produced stimuli, s, s, s. Consequently, when S_d recurs and the organism starts to perform R_i, the resulting stimuli "remind" the organism of the antecedent punishment, i.e., cue off r_f, which tends to inhibit R_i.

Two-factor learning theory, original version, thus accepted Thorndike's conception of habit formation but derived the phenomenon of punishment from fear conditioning, rather than attributing it to a process which simply reversed the effects of past reward. As has been shown, this theory of punishment involves the same principles as have been found most satisfactory for explaining so-called avoidance learning. In both instances, fear becomes condi-

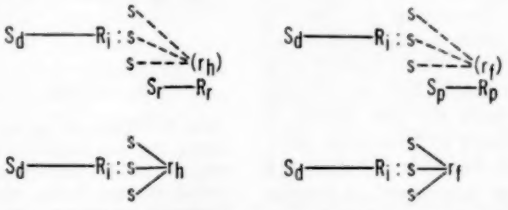

FIG. 6. Schematic representation of revised two-factor theory. Here the changes produced in behavior by reward, as well as those produced by punishment, are derived from conditioning plus the feedback principle.

tioned to stimuli, either external or response-produced, and the organism then makes whatever type of adjustment will most effectively eliminate these stimuli and reduce the attendant fear.

Revised two-factor theory is diagramed in Fig. 6. Here it is assumed that habit formation is a matter of conditioning no less than is punishment. If a stimulus, S_d, produces a given response R_t, and if R_t is followed by reward S_r, then it is assumed that a part of the total response, R_r, which is produced by S_r will become conditioned to the stimuli inherently connected with R_t. Here the conditionable component of R_r is r_h, the "hope" (secondary reinforcement) reaction; and it becomes connected to stimuli, s, s, s, just as fear does in the case of punishment. The result is that whenever R_t starts to occur, it is facilitated rather than inhibited.

As revised, two-factor theory holds that all learning is sign learning. The original version used sign learning to account only for the effects of punishment. Now it is proposed that the effects of reward are likewise to be explained in terms of sign learning, or conditioning. Thorndike's original formulation of the Law of Effect had held that reward *strengthens* the bond between S_d and R_t and that punishment *weakens* it. The present position is that neither rewards nor punishments affect this "connection," in any neurological sense, and that the attendant learning is a matter of positive or negative meanings—hopes or fears—getting connected to response-produced stimuli. If on the basis of past experience, the feedback from a given response is positive (hopeful), the response will be facilitated; if the feedback is negative (fearful), the response will be inhibited; [6] but in neither case is it assumed that learning involves the S_d—R_t connection.

Here it is assumed that S_d *drives,* or motivates, behavior but that specific actions are determined, not by the nature of specific S_d—R_t neurological bonds, but by the *guiding* (controlling) action of the r_h and r_f reactions which have become conditioned to response-produced stimuli (or their symbolic equivalents). It is not maintained that these two types of conditioning are necessarily the only kinds of learning that occur; but it is believed that they go a long way toward accounting for so-called selective learning and the determination of overt behavior in any given situation.

As previously indicated, this way of thinking about the effects of reward and punishment articulates well with the more general aspects of field theory. It is not possible here to develop this relationship in any detail; but if the reader will consult a diagram published by Tolman some twenty years ago (39, p. 380), he will find a high degree of compatibility between it and the drawings shown in Fig. 6. One may say that Thorndike's Law of Effect and Tolman's sign-gestalt theory have *seemed* to be more different than they really are, mainly because one stressed certain features of a common process and neglected others, while the other theory did the reverse. By making the effects of punishment and reward more fully explicit, as in Fig. 6, it is believed that a satisfactory reconciliation between habit theory and field theory can be achieved. Those conversant with psychoanalytic thought may also see a parallelism with the Freudian concepts of *cathexis* and *anti-cathexis* (8).

CRITICISMS AND COMMENT

Colleagues and students have kindly read a preliminary draft of this paper. Some of their criticisms follow:

[6] In this sentence, "positive" and "negative" are used, not as in cybernetics, but in a psychological sense.

1. *Two "factors" or one?*

Your two processes reduce to one: S-S contiguity. As you say, "In the present paper it is suggested that all learning is in the nature of sign learning" (p. 121). Why then speak of two processes? Both secondary motivation and secondary reinforcement (in your usage) develop out of the contiguous occurrence of two afferent processes—a neutral stimulus (interoceptive or exteroceptive) in conjunction with an increment or decrement in a drive-stimulus—and the first process thereby acquires some of the functional properties of the second. A unified S-S-contiguity formulation is not only neater, but more powerful as well, because it handles the case in which a neutral stimulus becomes the sign of another *neutral* stimulus, e.g., sensory preconditioning, association of ideas, and the like. It is more powerful, too, because it doesn't tie your learning theory to a drive-theory of motivation, which it seems to me must soon be abandoned in favor of a hedonic principle (e.g., P. T. Young's views).

Although the distinction between sign learning and solution learning has been abandoned, the alternative formulation posits two basically different types of reinforcement: drive reduction and drive induction. Since these are the same as were involved in the original version of two-factor theory, it does not seem amiss to refer to the revised formulation as involving two (reinforcement) "factors."

What is implied by a "unified S-S-contiguity formulation" has never been entirely clear to the present writer. It is true that revised two-factor theory posits only sign, or "contiguity," learning; but it is S-S-*r* rather than S-S learning, in which the "*r*" is conditioned fear or hope (or some similar "attitude" or emotion). In a sense, revised two-factor theory is *both* an S-S theory and an S-r theory, but it is neither alone. As indicated, it is most accurately represented as an S-S-r theory, which is believed capable of handling the salient facts of both "conditioning" and "habit formation."

Contiguity theory, as defined in the above quotation, seems to make over what is merely a necessary condition for learning into the sufficient one. As Osgood has pointed out, virtually all theories of learning agree "that *contiguity* is one necessary condition" (**29a**, p. 321). But is it really the sufficient one? Think of the infinite number of cases of repeated temporal conjunction of stimuli in which learning apparently does *not* occur. These negative instances make a pure stimulus-contiguity theory, not "powerful," but impotent. Contiguity *plus* reinforcement (significance) seem to be essential for even the most rudimentary conception of learning. So long as the evidence for sensory preconditioning or other empirical indications of learning without reinforcement (decremental *or* incremental) remains as scant and as ambiguous as it presently is, the S-S contiguity position remains unconvincing.

2. *Motivation vs. memory*

I . . . agree with your basic idea but think it a mistake to call it a theory of learning; it is a theory of motivation of action but seems to me irrelevant to the formation and retention of associations. I was recently served a very bad meal at a restaurant. I shall not go there again but I remember in detail the arrangement of the dining room, the appearance of the waitress, the flavor of the bad coffee, even the decorations on the dinner ware.

Motivational theories were developed from studies of animals, in which it is very difficult to demonstrate memory without motivating action. But even with rats one can observe bits of behavior which suggests that in the course of training the animal learns many things unrelated to the motivation of the experiment. Like me, the animal learns his motives but learns a great deal besides. I feel sure that the learning problem would be much clarified, if this distinction were emphasized. The key to learning is in the phenomena which Thorndike called 'Belongingness,' but I don't seem able to make any progress there.

Here the present writer has no very adequate answer. The basic issue is, again, one of fact: Can learning occur

on a "purely cognitive" basis or must there be a dynamic (motivational) element present? It would certainly be premature to deny the former possibility, but it is the latter assumption that has proven most fruitful research-wise. Moreover, an empirical study of word meanings reported recently by Osgood and Suci (30) indicates that the "good-bad" dimension in "semantic space" accounts for about 70 per cent of the total variance. The *connotations* of words (and other signs) are pretty clearly evaluative. It is when we come to the *denotative* (cognitive) function of signs that two-factor theory is most seriously challenged. While relevant, this problem lies beyond the scope of the present paper.

3. *Emotions other than hope and fear*

Are drive decrement (reward) and drive increment (punishment) the *only* reinforcers?

We know that nausea and certain other emotional states, besides hope and fear, are conditionable (17). Appetite and lustful excitement, on the one hand (33), and frustration and anger, on the other,[7] present still other relatively unexplored problems. But rewards and hopes, and punishments and fears, seem to constitute the *basic*, though not necessarily the *only*, forms of reinforcement and motivation.

4. *Conditioned fear reduction: an enigma*

You have posited, following Miller, that fear is an unconditioned response to pain [see Fig. 1]. You have also posited that secondary reinforcement (of the hope variety) involves a conditioned alleviation of fear [Fig. 2], which occurs when the unconditioned stimulus (or primary drive) is *still present*. Is it not somewhat illogical to posit a conditioned reaction that involves the lessening of an *un*conditioned reaction?

[7] Lawson, R., & Marx, M. H. Frustration: theories and experiments (to be published).

This is a nice point and one that may throw light upon some heretofore rather puzzling observations. The assumption which must obviously be made is that the fear aroused by a strong primary drive is partly reflexive (unconditioned) and partly learned (conditioned). Because drives commonly start out at low intensity and become stronger, they often have a *cue* function, in addition to their unconditional capacity for arousing fear. Therefore, we may infer that a "safety" signal or "promise" of relief can counteract the fear accompanying a strong drive *only* to the extent that the fear is learned rather than reflexive. Here may lie, in part, the explanation of why it is relatively easy to give secondary-reinforcement potential to stimuli that are associated with the reduction of hunger or thirst and harder to give it to a stimulus associated with termination of a more painful drive such as electric shock. This line of thought clearly calls for further development.

5. *Skill and discrimination*

Very often the remark has been made: "Granted that much, perhaps most, learning is sign learning, is it not probable that *skills* involve a form of purely 'motor' learning?" Keller (14) has offered a quite satisfactory answer to this question in his hypothesis that the perfection of motor skills is a matter of increasing refinement of discrimination between the *sensory feedback* from right and not-quite-right muscular performances (see also 1 and 9). Whatever the facts may ultimately prove to be, Keller's hypothesis seems *logically* adequate, within the present theoretical framework.

6. *Response initiation: another enigma*

Whatever you decide to do about the first problem [Paragraph 1], you still need another process (though not necessarily a learning process) to account for selective learning. As

it stands, your principle of secondary reinforcement is not enough. The fact that stimuli (intero- or exteroceptive) have been associated with hunger-reduction and thereby acquire a certain positive valence does not explain how these stimuli, even if they occur *before* the instrumental response which accomplishes the need reduction, come to elicit that response. How does "hope" lead to appropriate action? Once "R_1 *starts* to occur" (p. 123, italics mine), it can be inhibited or facilitated by conditioned drive increments or decrements—an assumption which should be set off more clearly, by the way—but why does it start? One possibility is that the organism is constantly scanning its response repertoire, with scanning rate proportional to drive level. Another possibility is to return to a process of stimulus-response connection. Neither appeals to me. But the problem of response elicitation must be recognized.

If one assumes, with Tolman, that learning consists solely of the establishment of new cognitions, then the issue of response initiation is indeed a serious one. As Guthrie (10) has observed:

In his concern with what goes on in the rat's mind, Tolman has neglected to predict what the rat will do. So far as the theory is concerned the rat is left buried in thought; if it gets to the food box at the end of the maze that is its concern, not the concern of the theory (10, p. 143).

Revised two-factor theory, in positing hope and fear as the principal products of learning, is surely a good deal more dynamic and less susceptible to the foregoing criticism than is sign-gestalt theory. In fact, one reader of the present paper has said:

One of the significant features of your new theory, which is not mentioned by you, is that it contains the seed, or the starting point, of a theory of behavior as such, of which a learning or habit theory is merely an offshoot. Your Diagram #6, although I have some definite criticisms of it, suggests to me a general paradigm of behavior. It is when it refers to recurring similar situations that it is also applicable to learning. Your willingness to deal with meanings rather than S-R connections and bonds gives your position a general applicability.

The reader first cited nevertheless has a point. If, as posited, behavior is constantly guided (inhibited and facilitated) by the sensory consequences (feedback) of behavior and the meaning (motivational) states they then produce, there is certainly the anterior question of how behavior *gets started*. Some initial, or *initiating*, drive is, of course, assumed; but the question is, Why should it initiate one response rather than any other?

Some extremely difficult problems are unquestionably involved here, but the arguments against the concept of scanning are not so convincing to the present writer as to the correspondent cited. Living organisms, when conscious and motivated, are constantly "scanning" their environment, with their exteroceptors, for "leads" or cues as to what to do (and not do). When the environment is cue-improvised, as it often is in maze experiments, the organism may be reduced to searching, or "scanning," for *proprioceptive* leads. This is sometimes done by performing certain responses, overtly but *in miniature*, as in VTE behavior; or, in higher organisms, the process may involve symbolic activities which are entirely covert. *How* an organism plays his receptors or how he engages in "pure-stimulus acts" (Hull, 11) is admittedly a mystery. But the *Why* is obvious: the organism is "looking" and "feeling" around for signs that will either release action which may be gratifying or will block action which might be harmful. Therefore, as of the present, it would appear that the concept of scanning provides what is perhaps our most promising lead in this connection. More detailed attention will be given to this problem at another time.

SUMMARY

Two-factor learning theory, in its original version, held that there are two

basically different learning processes: sign learning, conditioning, or stimulus substitution *and* solution learning, trial and error, or response substitution.

This point of view gained considerable strength from the fact that it handled the facts of punishment and avoidance learning more adequately than did the theories of Thorndike, Pavlov, and Hull. However, it was weak in that it continued to accept, essentially unmodified, Thorndike's conception of habit formation, as a matter of simple S-R "bonding"; and it did not take full cognizance of the emerging research on secondary reinforcement.

In the present paper, earlier thinking about fear conditioning and the reinforcing effects of fear reduction is extended in such a way as to provide, within the same conceptual framework, a new interpretation of secondary reinforcement. And with this step taken, it is found that a radical modification of the traditional concept of *habit* logically emerges.

The original version of two-factor theory differs from Thorndike's Law of Effect in holding that punishment, rather than directly weakening or "stamping-out" a neural connection, or "bond," achieves its action by causing fear to become conditioned to response-correlated stimuli, so that when the organism subsequently starts to repeat the punished response, fear is aroused, which causes conflict and response inhibition, or at least modification.

Revised two-factor theory, as here proposed, holds that habit formation is likewise dependent upon feedback, just as punishment is. If fear becomes conditioned to stimuli which are associated with a response that has been followed by punishment (drive increment), then *hope* (secondary reinforcement) becomes likewise connected to stimuli which are associated with a response that has been followed by reward (drive decrement). "Positive" feedback, rather than a motive-behavior "bond," is thus hypothesized as the essence of habit. Such a position readily articulates with modern servo-theory, field theory, and parts of psychoanalytic theory. Although employing different terminology, a number of other contemporary writers, it seems, are moving toward a somewhat similar point of view.

Examples of experimental evidence supporting this revised view of habit are briefly (and quite incompletely) cited, along with some complications and unsolved problems.

REFERENCES

1. BAHRICK, H. P., FITTS, P. M., & SCHNEIDER, R. The reproduction of simple movements as a function of proprioceptive feedback. *J. exp. Psychol.*, 1955, **49**, 437–444.
2. BIRCH, H. G., & BITTERMAN, M. E. Sensory integration and cognitive theory. *Psychol. Rev.*, 1951, **58**, 355–361.
3. BRECHER, E., & BRECHER, RUTH. The whys of aches and pains. *Reader's Digest*, April, 1955, 73–76.
4. BROWN, J. S., & JACOBS, A. The role of fear in the motivation and acquisition of response. *J. exp. Psychol.*, 1949, **39**, 749–759.
5. BUGELSKI, R. Extinction with and without sub-goal reinforcement. *J. comp. Psychol.*, 1938, **26**, 121–133.
6. DINSMOOR, J. A. A quantitative comparison of the discriminative and reinforcing functions of a stimulus. *J. exp. Psychol.*, 1950, 40, 458–472.
7. DINSMOOR, J. A. Punishment: I. The avoidance hypothesis. *Psychol. Rev.*, 1954, **61**, 34–46.

7a. ELITHORN, A., PIERCY, M. F., & CROSSLEY, MARGARET A. Prefrontal leucotomy and the anticipation of pain. *J. Neurol. Neurosurg. Psychiat. (London)*, 1955, **18**, 34–43.

8. FREUD, S. *A general introduction to psychoanalysis*. New York: Liveright, 1935.
9. GIBBS, C. G. The continuous regulation of skilled response by kinaesthetic feedback. *Brit. J. Psychol.*, 1954, **45**, 24–39.
10. GUTHRIE, E. R. *The psychology of learning*. (Rev. Ed.). New York: Harper, 1952.

11. HULL, C. L. Knowledge and purpose as habit mechanism. *Psychol. Rev.*, 1930, **37**, 511–525.
12. HULL, C. L. *Principles of behavior*. New York: Appleton-Century, 1943.
13. HUNTER, W. S. Conditioning and extinction in the rat. *Brit. J. Psychol.*, 1935, **26** (II), 135–148.
14. KELLER, F. S. Learning (reinforcement theory). In *Doubleday papers in psychology*. Garden City, N. Y.: Doubleday, 1954.
15. KONORSKI, J. *Conditioned reflexes and neural organization*. England: University Press, 1948.
16. KONORSKI, J. Mechanisms of learning. In *Physiological mechanisms in animal behavior*. New York: Academic Press, 1950.
17. LOUCKS, R. B. The experimental delimitation of structures essential for learning: The attempt to condition striped muscle responses with faradization of the sigmoid gyrus. *J. Psychol.*, 1935, **1**, 5–44.
18. MCCLELLAND, D. C. Toward a theory of motivation. In *The achievement motive*. New York: Appleton-Century-Crofts, 1953.
19. MCCLELLAND, D. C. Notes for a revised theory of motivation. In D. C. McClelland (Ed.), *Studies in motivation*. New York: Appleton-Century-Crofts, 1955.
20. MILLER, N. E. Studies of fear as an acquirable drive. I. Fear as motivation and fear-reduction as reinforcement in the learning of new responses. *J. exp. Psychol.*, 1948, **38**, 89–101.
21. MILLER, N. E. Learnable drives and rewards. In S. S. Stevens (Ed.), *Handbook of experimental psychology*. New York: Wiley, 1951.
22. MILLER, N. E., & DOLLARD, J. *Social learning and imitation*. New Haven: Yale Univer. Press, 1941.
23. MOWRER, O. H. A stimulus-response analysis of anxiety and its role as a reinforcing agent. *Psychol. Rev.*, 1939, **46**, 553–565.
24. MOWRER, O. H. On the dual nature of learning—a reinterpretation of "conditioning" and "problem solving." *Harvard educ. Rev.*, 1947, **17**, 102–148.
25. MOWRER, O. H. Further evidence for a two-factor theory of learning. In *Learning theory and personality dynamics*. New York: Ronald, 1950, Chap. 10.
26. MOWRER, O. H. A reinterpretation of four empirical studies. In *Learning theory and personality dynamics*. New York: Ronald, 1950, Chap. 12.
27. MOWRER, O. H. Motivation and neurosis. In *Nebraska Symposium*. Lincoln, Nebraska: Univer. of Nebraska Press, 1953.
28. MOWRER, O. H., & LAMOREAUX, R. R. Fear as an intervening variable in avoidance conditioning. *J. comp. Psychol.*, 1946, **39**, 29–50.
29. MUENZINGER, K. F. Reward and punishment. *Univer. Colorado Stud., General Ser.* (A), 1946, **27**, No. 4., pp. 1–16.
29a. OSGOOD, C. E. *Method and theory in experimental psychology*. New York: Oxford Univer. Press, 1953.
30. OSGOOD, C. E., & SUCI, G. J. Factor analysis of meaning. *J. exp. Psychol.*, 1955, **50**, 325–338.
31. PAVLOV, I. P. *Conditioned reflexes*. (Trans. by G. V. Anrep.) Oxford Univer. Press, 1927.
32. SEWARD, J. P. Note on the externalization of drive. *Psychol. Rev.*, 1942, **49**, 197–199.
33. SEWARD, J. P. Secondary reinforcement as tertiary motivation: A revision of Hull's revision. *Psychol. Rev.*, 1950, **57**, 362–374.
34. SEWARD, J. P. Experimental evidence for the motivating function of reward. *Psychol. Bull.*, 1951, **48**, 130–149.
35. SKINNER, B. F. *The behavior of organisms*. New York: Appleton-Century, 1938.
36. SLUCKIN, W. *Minds and machines*. London: Penguin Books, 1954.
37. SOLOMON, R. L., & WYNNE, L. C. Traumatic avoidance learning: the principles of anxiety conservation and partial irreversibility. *Psychol. Rev.*, 1954, **61**, 353–385.
38. TOLMAN, E. C. *Purposive behavior in animals and men*. New York: Appleton-Century, 1932.
39. TOLMAN, E. C. Theories of learning. In F. A. Moss (Ed.), *Comparative psychology*. New York: Prentice-Hall, 1934. Pp. 367–408.
40. TOLMAN, E. C. *Collected papers in psychology*. Berkeley: Univer. of California Press, 1951.
41. WIENER, N. *Cybernetics*. New York: Wiley, 1948.
42. WOODWORTH, R. S., & SCHLOSBERG, H. *Experimental psychology*. (Rev. Ed.) New York: Holt, 1954.

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RATIONAL CHOICE AND THE STRUCTURE OF THE ENVIRONMENT[1]

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A growing interest in decision making in psychology is evidenced by the recent publication of Edwards' review article in the *Psychological Bulletin* (1) and the Santa Monica Conference volume, *Decision Processes* (7). In this work, much attention has been focused on the characterization of *rational* choice, and because the latter topic has been a central concern in economics, the theory of decision making has become a natural meeting ground for psychological and economic theory.

A comparative examination of the models of adaptive behavior employed in psychology (e.g., learning theories), and of the models of rational behavior employed in economics, shows that in almost all respects the latter postulate a much greater complexity in the choice mechanisms, and a much larger capacity in the organism for obtaining information and performing computations, than do the former. Moreover, in the limited range of situations where the predictions of the two theories have been compared (see [7, Ch. 9, 10, 18]), the learning theories appear to account for the observed behavior rather better than do the theories of rational behavior.

Both from these scanty data and from an examination of the postulates of the economic models it appears probable that, however adaptive the behavior of organisms in learning and choice situations, this adaptiveness falls far short of the ideal of "maximizing" postulated in economic theory. Evidently, organisms adapt well enough to "satisfice"; they do not, in general, "optimize."

If this is the case, a great deal can be learned about rational decision making by taking into account, at the outset, the limitations upon the capacities and complexity of the organism, and by taking account of the fact that the environments to which it must adapt possess properties that permit further simplication of its choice mechanisms. It may be useful, therefore, to ask: How simple a set of choice mechanisms can we postulate and still obtain the gross features of observed adaptive choice behavior?

In a previous paper (6) I have put forth some suggestions as to the kinds of "approximate" rationality that might be employed by an organism possessing limited information and limited computational facilities. The suggestions were "hypothetical" in that, lacking definitive knowledge of the human decisional processes, we can only conjecture on the basis of our everyday experiences, our introspection, and a very limited body of psychological literature what these processes are. The suggestions were intended, however, as empirical statements, however tentative, about some of the actual mechanisms involved in human and other organismic choice.[2]

Now if an organism is confronted

[1] I am indebted to Allen Newell for numerous enlightening conversations on the subject of this paper, and to the Ford Foundation for a grant that permitted me leisure to complete it.

[2] Since writing the paper referred to I have found confirmation for a number of its hypotheses in the interesting and significant study, by A. de Groot, of the thought processes of chess players (3). I intend to discuss the implications of these empirical findings for my model in another place.

with the problem of behaving approximately rationally, or adaptively, in a particular environment, the kinds of simplifications that are suitable may depend not only on the characteristics—sensory, neural, and other—of the organism, but equally upon the structure of the environment. Hence, we might hope to discover, by a careful examination of some of the fundamental structural characteristics of the environment, some further clues as to the nature of the approximating mechanisms used in decision making. This is the line of attack that will be adopted in the present paper.

The environment we shall discuss initially is perhaps a more appropriate one for a rat than for a human. For the term *environment* is ambiguous. We are not interested in describing some physically objective world in its totality, but only those aspects of the totality that have relevance as the "life space" of the organism considered. Hence, what we call the "environment" will depend upon the "needs," "drives," or "goals" of the organism, and upon its perceptual apparatus.

THE ENVIRONMENT OF THE ORGANISM

We consider first a simplified (perhaps "simple-minded") organism that has a single need—food—and is capable of three kinds of activity: resting, exploration, and food getting. The precise nature of these activities will be explained later. The organism's life space may be described as a surface over which it can locomote. Most of the surface is perfectly bare, but at isolated, widely scattered points there are little heaps of food, each adequate for a meal.

The organism's vision permits it to see, at any moment, a circular portion of the surface about the point in which it is standing. It is able to move at some fixed maximum rate over the surface. It metabolizes at a given average rate and is able to store a certain amount of food energy, so that it needs to eat a meal at certain average intervals. It has the capacity, once it sees a food heap, to proceed toward it at the maximum rate of locomotion. The problem of rational choice is to choose its path in such a way that it will not starve.

Now I submit that a rational way for the organism to behave is the following: (*a*) it explores the surface at random, watching for a food heap; (*b*) when it sees one, it proceeds to it and eats (food getting); (*c*) if the total consumption of energy during the average time required, per meal, for exploration and food getting is less than the energy of the food consumed in the meal, it can spend the remainder of its time in resting.[3]

There is nothing particularly remarkable about this description of rational choice, except that it differs so sharply from the more sophisticated models of human rationality that have been proposed by economists and others. Let us see what it is about the organism and its environment that makes its choice so simple.

1. It has only a single goal: food. It does not need to weigh the respective advantages of different goals. It requires no "utility function" or set of "indifference curves" to permit it to choose between alternatives.

2. It has no problem of maximization. It needs only to maintain a certain average rate of food intake, and additional food is of no use to it. In

[3] A reader who is familiar with W. Grey Walter's mechanical turtle, *Machina speculatrix* (8), will see as we proceed that the description of our organism could well be used as a set of design specifications to assure the survival of his turtle in an environment sparsely provided with battery chargers. Since I was not familiar with the structure of the turtle when I developed this model, there are some differences in their behavior—but the resemblance is striking.

the psychologist's language, it has a definite, fixed aspiration level, and its successes or failures do not change its aspirations.

3. The nature of its perceptions and its environment limit sharply its planning horizon. Since the food heaps are distributed randomly, there is no need for pattern in its searching activities. Once it sees a food heap, it can follow a definite "best" path until it reaches it.

4. The nature of its needs and environment create a very natural separation between "means" and "ends." Except for the food heaps, one point on the surface is as agreeable to it as another. Locomotion has significance only as it is a means to reaching food.[4]

We shall see that the first point is not essential. As long as aspirations are fixed, the planning horizon is limited, and there is a sharp distinction between means and ends, the existence of multiple goals does not create any real difficulties in choice. The real complications ensue only when we relax the last three conditions; but to see clearly what is involved, we must formulate the model a little more precisely.

PERCEPTUAL POWERS, STORAGE CAPACITY, AND SURVIVAL

It is convenient to describe the organism's life space not as a continuous surface, but as a branching system of paths, like a maze, each branch point representing a choice point. We call the selection of a branch and locomotion to the next branch point a "move." At a small fraction of the branch points are heaps of food.

Let p, $0<p<1$, be the percentage of branch points, randomly distributed, at which food is found. Let d be the average number of paths diverging from each branch point. Let v be the number of moves ahead the organism can see. That is, if there is food at any of the branch points within v moves of the organism's present position, it can select the proper paths and reach it. Finally let H be the maximum number of moves the organism can make between meals without starving.

At any given moment, the organism can see d branch points at a distance of one move from his present position, d^2 points two moves away, and in general, d^k points k moves away. In all, it can see

$$d + d^2 + \cdots + d^v = \frac{d}{d-1}(d^v - 1)$$

points. When it chooses a branch and makes a move, d^v new points become visible on its horizon. Hence, in the course of m moves, md^v new points appear. Since it can make a maximum of H moves, and since v of these will be required to reach food that it has discovered on its horizon, the probability, $Q = 1 - P$, that it will *not* survive will be equal to the probability that no food points will be visible in $(H - v)$ moves. (If p is small, we can disregard the possibility that food will be visible inside its planning horizon on the first move.) Let ρ be the probability that none of the d^v new points visible at the end of a particular move is a food point.

$$\rho = (1 - p)^{d^v}. \qquad [2.1]$$

Then:

$$1 - P = Q = \rho^{(H-v)} = (1 - p)^{(H-v)d^v}. \qquad [2.2]$$

We see that the survival chances, from meal to meal, of this simple organism depend on four parameters, two that describe the organism and two the environment: p, the richness of the environment in food; d, the richness of the environment in paths; H, the storage capacity of the organism; and v, the range of vision of the organism.

[4] It is characteristic of economic models of rationality that the distinction between "means" and "ends" plays no essential role in them. This distinction *cannot* be identified with the distinction between behavior alternatives and utilities, for reasons that are set forth at some length in the author's *Administrative Behavior*, Ch. 4 and 5 (5).

To give some impression of the magnitudes involved, let us assume that p is 1/10,000, $(H - v)$ is 100, d is 10 and v is 3. Then the probability of seeing a new food point after a move is $1 - \rho = 1 - (1 - p)^{1000} \sim 880/10{,}000$, and the probability of survival is $P = 1 - \rho^{100} \sim 9999/10{,}000$. Hence there is in this case only one chance in 10,000 that the organism will fail to reach a food point before the end of the survival interval. Suppose now that the survival time $(H - v)$ is increased one-third, that is, from 100 to 133. Then a similar computation shows that the chance of starvation is reduced to less than one chance in 100,000. A one-third increase in v will, of course, have an even greater effect, reducing the chance of starvation from one in 10^{-4} to one in 10^{-40}.

Using the same values, $p = .0001$, and $(H - v) = 100$, we can compute the probability of survival if the organism behaves completely randomly. In this case $P' = [1 - (1 - p)^{100}] = .009$. From these computations, we see that the organism's modest capacity to perform purposive acts over a short planning horizon permits it to survive easily in an environment where random behavior would lead to rapid extinction. A simple computation shows that its perceptual powers multiply by a factor of 880 the average speed with which it discovers food.

If p, d, and v are given, and in addition we specify that the survival probability must be greater than some number close to unity $(P \geq 1 - \epsilon)$, we can compute from [2.2] the corresponding minimum value of H:

$$\log (1 - P) = (H - v) \log \rho \quad [2.3]$$

$$H \geq v + \frac{\log \epsilon}{\log \rho}. \quad [2.4]$$

For example, if $\rho = .95$ and $\epsilon = 10^{-10}$, then $\log \rho = -.022$, $\log \epsilon = -10$ and $(H - v) \geq 455$. The parameter, H, can be interpreted as the "storage capacity" of the organism. That is, if the organism metabolizes at the rate of α units per move, then a storage of αH food units, where H is given by Equation 4, would be required to provide survival at the specified risk level, ϵ.

Further insight into the meaning of H can be gained by considering the average number of moves, M, required to discover food. From Equation 1, the probability of making $(k - 1)$ moves without discovering food, and then discovering it on the k^{th} is:

$$P_k = (1 - \rho)\rho^{(k-1)}. \quad [2.5]$$

Hence, the average number of moves, M, required to discover food is:

$$M = \sum_{k=1}^{\infty} k(1 - \rho)\rho^{k-1}$$

$$= \frac{(1 - \rho)}{(1 - \rho)^2} = \frac{1}{(1 - \rho)}. \quad [2.6]$$

Since $(1 - \rho)$ is the probability of discovering food in any one move, M is the reciprocal of this probability. Combining [2.3] and [2.6], we obtain:

$$\frac{M}{H - v} = \frac{\log \rho}{(1 - \rho) \log (1 - P)}. \quad [2.7]$$

Since ρ is close to one, $\log_e \rho \simeq (1 - \rho)$, and [2.7] reduces approximately to:

$$\frac{M}{H - v} \simeq \frac{1}{\log_e (1 - P)}. \quad [2.8]$$

For example, if we require $(1 - P) = \epsilon \leq 10^{-4}$ (one chance in 10,000 of starvation), then $M/(H - v) \leq .11$. For this survival level we require food storage approximately equal to $\alpha(v + 9M)$—food enough to sustain the organism for nine times the period required, on the average, to discover food, plus the period required to reach the food.[5]

[5] I have not discovered any very satisfactory data on the food storage capacities of animals, but the order of magnitude suggested above for the ratio of average search time to storage capacity is certainly correct. It may be noted that, in some cases at least, where the "food" substance is ubiquitous, and hence the search time negligible, the storage capacity is also small.

CHOICE MECHANISMS FOR MULTIPLE GOALS

We consider now a more complex organism capable of searching for and responding to two or more kinds of goal objects. In doing this we could introduce any desired degree of complexity into the choice process; but the interesting problem is how to introduce multiple goals with a minimum complication of the process—that is, to construct an organism capable of handling its decision problems with relatively primitive choice mechanisms.

At the very least, the presence of two goals will introduce a consistency requirement—the time consumed in attaining one goal will limit the time available for pursuit of the other. But in an environment like the one we have been considering, there need be no further relationship between the two goals. In our original formulation, the only essential stipulation was that H, the storage capacity, be adequate to maintain the risk of starvation below a stipulated level $(1 - P)$. Now we introduce the additional stipulation that the organism should only devote a fraction, λ, of its time to food-seeking activities, leaving the remaining fraction, $1 - \lambda$, to other activities. This new stipulation leads to a requirement of additional storage capacity.

In order to control the risk of starving, the organism must begin its exploration for food whenever it has reached a level of H periods of food storage. If it has a total storage of $(\mu + H)$ periods of food, and if the food heaps are at least $\alpha(\mu + H)$ in size, then it need begin the search for food only μ periods after its last feeding. But the food research will require, on the average, M periods. Hence, if a hunger threshold is established that leads the organism to begin to explore μ periods after feeding, we will have:

$$\lambda = \frac{M}{M + \mu}. \qquad [3.1]$$

Hence, by making μ sufficiently large, we can make λ as small as we please. Parenthetically, it may be noted that we have here a close analogue to the very common two-bin system of controlling industrial inventories. The primary storage, H, is a buffer stock to meet demands pending the receipt of new orders (with risk, $1 - P$, of running out); the secondary storage, μ, defines the "order point"; and $\mu + M$ is the average order quantity. The storage μ is fixed to balance storage "costs" against the cost (in this case, time pressure) of too frequent reordering.

If food and the second goal object (water, let us say) are randomly and independently distributed, then there are no important complications resulting from interference between the two activities. Designate by the subscript 1 the variables and parameters referring to food getting (e.g., μ_1 is the food threshold in periods), and by the subscript 2 the quantities referring to water seeking. The organism will have adequate time for both activities if $\lambda_1 + \lambda_2 < 1$.

Now when the organism reaches either its hunger or thirst threshold, it will begin exploration. We assume that if *either* of the goal objects becomes visible, it will proceed to that object and satisfy its hunger or thirst (this will not increase the number of moves required, on the average, to reach the other object); but if *both* objects become visible at the same time, and if S_1 and S_2 are the respective quantities remaining in storage at this time, then it will proceed to food or water as M_1/S_1 is greater or less than M_2/S_2. This choice will maximize its survival probability. What is required, then, is a mechanism that produces a drive proportional to M_i/S_i.

Thus, in terrestrial animals there is little oxygen storage and life can be maintained in the absence of air for only a few minutes. I am not arguing as to which way the causal arrow runs, but only that the organisms, in this respect, are adapted to their environments and do not provide storage that is superfluous.

A priority mechanism of the kind just described is by no means the only or simplest one that can be postulated. An even simpler rule is for the organism to persist in searching for points that will satisfy the particular need that first reached its threshold and initiated exploratory behavior. This is not usually an efficient procedure, from the standpoint of conserving goal-reaching time, but it may be entirely adequate for an organism generously endowed with storage capacity.

We see that an organism can satisfy a number of distinct needs without requiring a very elaborate mechanism for choosing among them. In particular, we do not have to postulate a utility function or a "marginal rate of substitution."

We can go even further, and assert that a primitive choice mechanism is adequate to take advantage of important economies, if they exist, which are derivable from the interdependence of the activities involved in satisfying the different needs. For suppose the organism has n needs, and that points at which he can satisfy each are distributed randomly and independently through the environment, each with the same probability, p. Then the probability that no points satisfying *any* of the needs will be visible on a particular move is ρ^n, and the mean number of moves for discovery of the *first* need-satisfying point is:

$$m_n = \frac{1}{(1-\rho^n)}. \qquad [3.2]$$

Suppose that the organism begins to explore, moves to the first need-satisfying point it discovers, resumes its exploration, moves to the next point it discovers that satisfies a need other than the one already satisfied, and so on. Then the mean time required to search for all n goals will be:

$$M_n = m_n + m_{n-1} + \cdots = \sum_{i=1}^{n} \frac{1}{(1-\rho^i)} \ll \frac{n}{(1-\rho)}. \qquad [3.3]$$

In particular, if ρ is close to one, that is, if need-satisfying points are rare, we will have:

$$M_n - M_{n-1} = \frac{1}{(1-\rho^n)} = \frac{1}{(1-\rho)} \cdot \frac{1}{\sum_{i=0}^{n} \rho^i} \simeq \frac{M_1}{n}, \qquad [3.4]$$

and

$$M_n \simeq M_1 \sum_{i=1}^{n} \frac{1}{i}. \qquad [3.5]$$

Now substituting particular values for n in [3.5] we get: $M_2 = 3/2\ M_1$; $M_3 = 11/6\ M_1$; $M_4 = 25/12\ M_1$, etc. We see that if the organism has two separate needs, its exploration time will be only 50 per cent greater than—and not twice as great as—if it has only one need; for four needs the exploration time will be only slightly more than twice as great as for a single need, and so on. A little consideration of the program just described will show that the joint exploratory process does not reduce the primary storage capacity required by the organism but does reduce the secondary storage capacity required. As a matter of fact, there would be no necessity at all for secondary storage.

This conclusion holds only if the need-satisfying points are *independently* distributed. If there is a negative correlation in the joint distribution of points satisfying different needs, then it may be economical for the organism to pursue its needs separately, and hence to have a simple signaling mechanism, involving secondary storage, to trigger its several exploration drives. This point will be developed further in the next section.

A word may be said here about "avoidance needs." Suppose that certain points in the organism's behavior space are designated as "dangerous." Then it will need to avoid those paths that lead to these particular points. If r per cent of all points, randomly distributed, are dangerous, then the number of available paths, among those visible

at a given move, will be reduced to $(1 - r)d^v$. Hence, $\rho' = (1 - p)^{(1-r)d^v}$ will be smaller than ρ (Equation 2.1), and M (Equation 2.6) will be correspondingly larger. Hence, the presence of danger points simply increases the average exploration time and, consequently, the required storage capacity of the organism.

FURTHER SPECIFICATION OF THE ENVIRONMENT: CLUES

In our discussion up to the present point, the range of the organism's anticipations of the future has been limited by the number of behavior alternatives available to it at each move (d), and the length of the "vision" (v). It is a simple matter to introduce into the model the consequences of several types of learning. An increase in the repertoire of behavior alternatives or in the length of vision can simply be represented by changes in d and v, respectively.

A more interesting possibility arises if the food points are not distributed completely at random, and if there are clues that indicate whether a particular intermediate point is rich or poor in paths leading to food points. First, let us suppose that on the path leading up to each food point the k preceding choice points are marked with a food clue. Once the association between the clue and the subsequent appearance of the food point is learned by the organism, its exploration can terminate with the discovery of the clue, and it can follow a determinate path from that point on. This amounts to substituting $v' = (v + k)$ for v.

A different kind of clue might operate in the following fashion. Each choice point has a distinguishable characteristic that is associated with the probability of encountering a food point if a path is selected at random leading out of this choice point. The organism can then select at each move the choice point with the highest probability. If only certain choice points are provided with such clues, then a combination of random and systematic exploration can be employed. Thus the organism may be led into "regions" where the probability of goal attainment is high relative to other regions, but it may have to explore randomly for food within a given region.

A concrete example of such behavior in humans is the "position play" characteristic of the first phase of a chess game. The player chooses moves on the basis of certain characteristics of resulting positions (e.g., the extent to which his pieces are developed). Certain positions are adjudged richer in attacking and defensive possibilities than others, but the original choice may involve no definite plan for the subsequent action after the "good" position has been reached.

Next, we turn to the problem of choice that arises when those regions of the behavior space that are rich in points satisfying one need (p_1 is high in these regions) are poor in points satisfying another need (p_2 is low in these same regions). In the earlier case of goal conflict (two or more points simultaneously visible mediating different needs), we postulated a priority mechanism that amounted to a mechanism for computing relative need intensity and for responding to the more intense need. In the environment with clues, the learning process would need to include a conditioning mechanism that would attach the priority mechanism to the response to competing clues, as well as to the response to competing visible needs.

Finally, we have thus far specified the environment in such a way that there is only one path leading to each point. Formally, this condition can always be satisfied by representing as two or more points any point that can be reached by multiple paths. For some

purposes, it might be preferable to specify an environment in which paths converge as well as diverge. This can be done without disturbing the really essential conditions of the foregoing analysis. For behavior of the sort we have been describing, we require of the environment only:

1. that if a path is selected *completely* at random the probability of survival is negligible;

2. that there exist clues in the environment (either the actual visibility of need-satisfying points or anticipatory clues) which permit the organism, sufficiently frequently for survival, to select specific paths that lead with certainty, or with very high probability, to a need-satisfying point.

CONCLUDING COMMENTS ON MULTIPLE GOALS

The central problem of this paper has been to construct a simple mechanism of choice that would suffice for the behavior of an organism confronted with multiple goals. Since the organism, like those of the real world, has neither the senses nor the wits to discover an "optimal" path—even assuming the concept of optimal to be clearly defined—we are concerned only with finding a choice mechanism that will lead it to pursue a "satisficing" path, a path that will permit satisfaction at some specified level of all of its needs.

Certain of the assumptions we have introduced to make this possible represent characteristics of the organism. (*a*) It is able to plan short purposive behavior sequences (of length not exceeding v), but not long sequences. (*b*) Its needs are not insatiable, and hence it does not need to balance marginal increments of satisfaction. If all its needs are satisfied, it simply becomes inactive. (*c*) It possesses sufficient storage capacity so that the exact moment of satisfaction of any particular need is not critical.

We have introduced other assumptions that represent characteristics of the environment, the most important being that need satisfaction can take place only at "rare" points which (with some qualifications we have indicated) are distributed randomly.

The most important conclusion we have reached is that blocks of the organism's time can be allocated to activities related to individual needs (separate means-end chains) without creating any problem of over-all allocation or coordination or the need for any general "utility function." The only scarce resource in the situation is time, and its scarcity, measured by the proportion of the total time that the organism will need to be engaged in *some* activity, can be reduced by the provision of generous storage capacity.

This does not mean that a more efficient procedure cannot be constructed, from the standpoint of the total time required to meet the organism's needs. We have already explored some simple possibilities for increasing efficiency by recognizing complementarities among activities (particularly the exploration activity). But the point is that these complications are not essential to the survival of an organism. Moreover, if the environment is so constructed (as it often is in fact) that regions rich in possibilities for one kind of need satisfaction are poor in possibilities for other satisfactions, such efficiencies may not be available.

It may be objected that even relatively simple organisms appear to conform to efficiency criteria in their behavior, and hence that their choice mechanisms are much more elaborate than those we have described. A rat, for example, learns to take shorter rather than longer paths to food. But this observation does not affect the cen-

tral argument. We can introduce a mechanism that leads the organism to choose time-conserving paths, where multiple paths are available for satisfying a given need, without any assumption of a mechanism that allocates time among *different* needs. The former mechanism simply increases the "slack" in the whole system, and makes it even more feasible to ignore the complementarities among activities in programming the over-all behavior of the organism.

This is not the place to discuss at length the application of the model to human behavior, but a few general statements may be in order. First, the analysis has been a static one, in the sense that we have taken the organism's needs and its sensing and planning capacities as given. Except for a few comments, we have not considered how the organism develops needs or learns to meet them. One would conjecture, from general observation and from experimentation with aspiration levels, that in humans the balance between the time required to meet needs and the total time available is maintained by the raising and lowering of aspiration levels. I have commented on this point at greater length in my previous paper.[6]

Second, there is nothing about the model that implies that the needs are physiological and innate rather than sociological and acquired. Provided that the needs of the organism can be specified at any given time in terms of the aspiration levels for the various kinds of consummatory behavior, the model can be applied.

The principal positive implication of the model is that we should be skeptical in postulating for humans, or other organisms, elaborate mechanisms for choosing among diverse needs. Common denominators among needs may simply not exist, or may exist only in very rudimentary form; and the nature of the organism's needs in relation to the environment may make their nonexistence entirely tolerable.

There is some positive evidence bearing on this point in the work that has been done on conflict and frustration. A common method of producing conflict in the laboratory is to place the organism in a situation where: (*a*) it is stimulated to address itself simultaneously to alternative goal-oriented behaviors, or (*b*) it is stimulated to a goal-oriented behavior, but restricted from carrying out the behaviors it usually evinces in similar natural situations. This suggests that conflict may arise (at least in a large class of situations) from presenting the animal with situations with which it is not "programmed" to deal. Conflict of choice may often be equivalent to an absence of a choice mechanism in the given situation. And while it may be easy to create such situations in the laboratory, the absence of a mechanism to deal with them may simply reflect the fact that the organism seldom encounters equivalent situations in its natural environment.[7]

CONCLUSION

In this paper I have attempted to identify some of the structural characteristics that are typical of the "psychological" environments of organisms. We have seen that an organism in an environment with these characteristics requires only very simple perceptual and choice mechanisms to satisfy its several needs and to assure a high probability of its survival over extended periods of

[6] See (6, pp. 111, 117–18). For an experiment demonstrating the adjustment of the rat's aspiration levels to considerations of realizability, see Festinger (2).

[7] See, for example, Neal E. Miller, "Experimental Studies of Conflict" in (4, Ch. 14).

time. In particular, no "utility function" needs to be postulated for the organism, nor does it require any elaborate procedure for calculating marginal rates of substitution among different wants.

The analysis set forth here casts serious doubt on the usefulness of current economic and statistical theories of rational behavior as bases for explaining the characteristics of human and other organismic rationality. It suggests an alternative approach to the description of rational behavior that is more closely related to psychological theories of perception and cognition, and that is in closer agreement with the facts of behavior as observed in laboratory and field.

REFERENCES

1. EDWARDS, W. The theory of decision making. *Psychol. Bull.*, 1954, **51**, 380–417.
2. FESTINGER, L. Development of differential appetite in the rat. *J. exp. Psychol.*, 1953, **32**, 226–234.
3. DE GROOT, A. *Het Denken van den Schaker.* Amsterdam: Noord-Hollandsche Uitgevers Maatschapij, 1946.
4. HUNT, J. McV. *Personality and the behavior disorders.* New York: Ronald, 1944.
5. SIMON, H. A. *Administrative behavior.* New York: Macmillan, 1947.
6. SIMON, H. A. A behavioral model of rational choice. *Quart. J. Econ.*, 1955, **59**, 99–118.
7. THRALL, R. M., COOMBS, C. H., & DAVIS, R. L. (Eds.). *Decision processes.* New York: Wiley, 1954.
8. WALTER, W. G. *The living brain.* New York: Norton, 1953.

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APPLICATION OF THE STATISTICAL THEORY TO FORM PERCEPTION

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As the contrast between a foveally viewed figure and its background is gradually increased, the appearance of the figure passes through a number of stages before it emerges with an unmistakable shape (2, 5, 12). In the case of an evenly illuminated figure viewed against a homogeneously dark ground, the most that can be seen at very low contrast is a blurred patch of light. During this initial perceptual stage which occurs at the threshold for light, the light patch appears formless (12) or, at the most, vaguely circular (26), irrespective of the true shape of the figure. At a brightness approximately 15 times as great as that necessary for the sensation of light the figure is perceived, usually inaccurately, with a shape other than its true shape. A particular "wrong" response, however, is frequently given with great consistency (2, 12). There are, of course, intervening stages in this sequence of events, but for the sake of convenience it is sufficient to regard it as a three-stage process referring thus to light threshold, threshold for indefinite form, and threshold for definite form.

In order to account for data of this nature at the neurophysiological level, a recent paper (2) has proposed a "diffusion" hypothesis based upon the now well-known satiation theory of Köhler and Wallach (16), originally put forward to interpret figural aftereffect data. An important objection to such an hypothesis can be found in the lack of correspondence between the alleged diffusion and satiation processes and the established evidence regarding neurophysiological events at the cortex. Dissatisfaction arising from this inconsistency has led recently (21) to a reinterpretation of the figural aftereffect in terms of a more orthodox theory of cortical activity.[1] This reinterpretation, which has its origins in the work of Marshall and Talbot (19), has as its central notion a cortical representation of a perceived contour in the form of a Gaussian distribution of neural excitation in area 17. This view, which will be referred to here as the statistical theory, is in sharp contrast to the first, which employs the notion of a potential difference between cortical areas of different ionic concentration giving rise to a flow of current about the cortical representation of a contour.

The same objections which led to an alternative interpretation of the figural aftereffect underlie the present attempt to account neurophysiologically for the phenomena of form perception outlined above. It is almost a truism to state that any hypothesis that seeks a relationship between subjective phenomena and central processes should take into consideration, as far as possible, what has been established regarding the nature of cortical and subcortical activity. The aim of this analysis will be, then, to interpret form-perception phenomena occurring at a near threshold contrast in terms of the statistical theory. If this can be achieved successfully, the

[1] Smith (24) has raised a number of objections to an interpretation of the figural aftereffect using the statistical theory (21). These objections apply only, however, to the use of this theory in relation to the figural aftereffect, and do not raise controversial issues in the application of this theory to the data treated here.

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REFERENCES

1. EDWARDS, W. The theory of decision making. *Psychol. Bull.*, 1954, **51**, 380–417.
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7. THRALL, R. M., COOMBS, C. H., & DAVIS, R. L. (Eds.). *Decision processes.* New York: Wiley, 1954.
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interests of parsimony will have been served and the way cleared, perhaps, for a neurophysiological approach to more complex data of visual perception.

The Statistical Theory

The statistical theory, as originally conceived of by Marshall and Talbot (19), deals with certain aspects of visual resolution from a neurophysiological viewpoint. Examination of a considerable body of neurophysiological, anatomical, and subjective evidence has led to a general theory of sensory acuity in its various forms (vernier, hairline, bar resolution), pattern illusions and depth discrimination. The theory has broken away from the classical interpretation of acuity in terms of the behavior of single receptors, and employs the notion of populations of neural elements behaving in a statistical fashion. The statistical theory emphasizes at the same time the role of the postretinal system. Visual acuity and contour recognition are functions attributable largely to the visual cortex and its associated systems.

Central to the theory is the concept of lateral and vertical summation. The former occurs across cells at the same neural level as, for instance, between bipolars, whereas vertical summation takes place between one neural level and another higher in the projection system, e.g., between receptors and bipolars and, presumably, between bipolar and ganglion elements. Lateral summation occurs most markedly in the periphery, although this process is by no means confined to this region. Thus Marshall and Talbot state, "The difference between central and peripheral organization is, even in this respect, merely a matter of degree or extensiveness" (19, p. 123). Ample evidence for this relative difference between foveal and peripheral organization has been adduced by Polyak (23). Vertical summation has its origin in the tendency for neurons to be fired, not by stimulation of a single contact, but by several impulses arriving at one or two contacts within a short time interval.

The principle of reciprocal overlap is of considerable importance in the summation processes and to the theory as a whole. This is described by the authors in the following manner:

> A sub-maximal reaction involving only part of the available neurons will have the greatest reaction density at the center of the reacting group. That is, on a numerical basis, more contacts are involved there. This results in a statistical peaking on the basis of a number of elements involved per unit cross-sectional area (19, p. 126).

Two further characteristics of the system are temporal and spatial dispersion of the retinal image. Temporal discontinuity results from certain features of the neural recovery cycle which break up neural activity, and from closed neural circuits which produce still further dispersion within the projection system. Spatial dispersion is a consequence of reciprocal overlap of pathways, some of which bear a cellular ratio of 1: 100 to each other. It is these properties of the primary projection system which, in the view of the authors, has necessitated a reorientation of the classical conception of isomorphic relations between receptor cells and cortex.[2]

Still further complexities are introduced by muscular and optical proper-

[2] In critically discussing the statistical theory, Byram (3) has stated that the proposals of this theory would demand a process analogous to that which would be involved in obtaining a sharp enlargement from a blurred photographic negative by manipulating the focusing mechanism of the enlarger so as to obtain an equal and opposite blur. In view of the complexities of the whole visual system, there is no reason to suppose that some such process could not occur. Indeed, the fineness of cortical grain, as compared with that of the retina, and the processes of vertical and lateral summation and damping within the projection system would appear to be suitable for accomplishing this.

ties of the eye. Physiological nystagmus produces eye flutter of an amplitude varying from 2′ (four cones) from 10 to 100 times per sec. to 30′ (60 cones) once per sec. In consequence, the neural "image" plays continuously across the projection area at the cortex. Marshall and Talbot describe this process:

> Physiologically, irregularly distributed nystagmus works to refine the image through this dynamic averaging process, without necessarily invoking the aid of neural recovery cycle behavior. A fine line oscillating over 4 or 5 rows of receptors would, even under equilibration conditions, produce a center of gravity through the action of partially shifted overlapping connections (19, p. 139).

Aberrations of the pupil, especially that of diffraction, cause a blurredness of the retinal image, so that there occurs a near normal distribution of intensity at each narrow line or sharp edge. The final outcome of these summation and dispersive processes within the primary projection system and the diffusion of the image at the eye is a statistical (Gaussian) distribution of excitation at the striate cortex. The synthesis of these diverse yet closely related activities is best described by the authors:

> The neural "image" plays continuously over the projection area at every synaptic level, building up gradients and peaks of activation at every edge and line. This is inherent in partially shifted reciprocal overlap. Multiplication of path both increases the reciprocal overlap and refines the mosaic in proportion to the sharper gradients and peaks produced, as sand forms sharper peaks than bricks (19, p. 139).

In order to employ this theory in an interpretation of the figural aftereffect, Osgood and Heyer (21) have made six further assumptions. Some of these are merely more clearly specified assumptions already implicit in the theory; others are extra to the original formulation of the theory. Here only those further assumptions that are necessary in dealing with form-perception data will be restated. The first assumption is a clarification of the notion of normal distribution of excitation at the cortex; the second is less implicit in Marshall and Talbot's paper, but would appear to be required for their theory; and the third and sixth assumptions have been made for the purpose of treating the figural aftereffect. These last two are necessary also for an interpretation of form perception data.

1. The representation of a contour in the projection cortex (area 17) is a normal distribution of excitation, symmetrical about its axis transversely, and extending as a "ridge" throughout the longitudinal extent of the contour.

2. "On-off type" fibers and their central connections are chiefly responsible for the distributions of excitation in area 17, which represent visual forms, lines, and contours.

3. The rate of excitation of the mechanisms responsible for contour perception will vary directly with (*a*) their nearness to intensity gradients on the retina, and (*b*) the sharpness of such intensity gradients.

6. The apparent localization of a contour in subjective visual space coincides with the location of maximal excitation in area 17.

THE STATISTICAL THEORY AND FORM PERCEPTION

It is now possible to link together the phenomenological reports of form perception with which this paper began and the statistical theory of neurophysiological activity in the visual system. During the first perceptual stage, that of an awareness of light, where contrast is low, and usually after the observer has been subjected to a period of dark adaptation (2, 5, 12), lateral summation would predominate and vertical summation would be at a minimum. It will be

recalled that Marshall and Talbot have indicated that differences with respect to foveal and peripheral relations between receptors, their bipolars, and ganglion cells are mainly a matter of degree, so that lateral summation doubtless occurs under the conditions of foveal viewing at low brightness. In the absence of a strong vertical component, statistical peaking of the excitation of distribution will be less marked than under conditions of greater brightness contrast. In consequence, the normal distribution of activity in area 17 will be flattened or platykurtic, since both the slope of the excitation gradient and the height of the distribution representing rate of excitation about the cortical locus of the contour edge will be slight. The probable nature of this distribution at the light threshold is shown in Fig. 1*a*. The form of the distribution follows from assumption 3 above, which states that the rate of excitation is dependent upon the nearness and sharpness of intensity gradients. Since intensity is low at this stage, and since the gradient is far from sharp, retinal and higher-level excitation will be weak.

At this point it seems reasonable to modify assumption 6 above, in order to take into account form perception at low brightnesses, with which Osgood and Heyer's analysis was not concerned. This is to the effect that (6*a*) the apparent localization of a contour in subjective visual space coincides with the location of maximal excitation in area 17 *when the slope and height of the excitation gradient exceeds a certain minimum value*. The slight slope and flattened distribution shown in Fig. 1*a* is not such as to permit contour localization, since the effective maximum excitation extends across a relatively wide band. The curve does not rise sufficiently sharply to delineate an edge between the two regions of different brightness. Marshall and Talbot have stated the possibility that the perception of light as contrasted with that of form is mediated by a separate system (19, p. 131). Here the "on-type" or "X-type" (9) ganglion fibers would be likely to maintain a succession of impulses during continuous photic stimulation, serving to evaluate brilliance over larger areas and serving probably an essentially protopathic function. As is characteristic of protopathic sensibility, there is an all-or-none response with no grading of the response (22), as would be required for the perception of form. Also, during this initial perceptual stage when the "on-type" fibers are presumably active in the absence of clear edge or contour, it would be likely that neural accommodation processes (13) would lead to a raising of the threshold for firing of those groups of cells most actively engaged, i.e., those at the center of the distribution, resulting in still further flattening of the excitation distribution. Thus there is a stage (light threshold) in form perception, under the conditions being considered here, at which borders and edges are not perceived, and probably at which vision is essentially protopathic rather than epicritic. The extent of excitation at this stage would be such as to produce a response to light, although (because of the flattened and unpeaked distribution) insufficient for even the vaguest hint of contour.

Within the context of the above argument may well be found the answer as to what is perceived first—light without form, or light with circular form. The historic but unresolved controversy (11, 12, 26) concerning whether the initial perception of light possesses or does not possess form qualities has never been satisfactorily concluded. It seems reasonable to suggest, in the light of the preceding discussion, that the formless impression of light is an earlier perceptual stage than that of the impression of

light with circular form. The first would be cortically represented by a less steep gradient and a more flattened excitation distribution than the second impression, which would occur later in the perceptual process. The appearance of light with circular form could also occur when increments in the contrast between figure and surround are of greater magnitude, so that the formless light stage would be left out of the sequence of perceptual events.

Increasing the brightness of a figure to about 15 times that required for the perception of light results in the impression of vague and indefinite form. At this stage it has been shown (2, 12) that the observer tends to mistake fairly consistently the shape of the stimulus figure reporting a square as a circle, an H-shape as a square or butterfly, and an L-shape as a triangle or semicircle. Since at this stage the brightness of the figure has increased, to give greater contrast it would be expected that the vertical component of the summation process would play a more active role. In consequence, the slope of the excitation distribution will become steeper, and the form of the distribution less flattened. As a result of clearer differentiation of figure and ground, there will occur an increase in the degree of nystagmatic flutter across the still ill-defined edges. Subjective reports indicate that the slope and height of the excitation curve is now at, or just above, the minimum required for the localization of edges and borders, but insufficiently in excess to attribute definite form qualities to the stimulus figure. As will be shown later, attentive processes probably play an important part between this stage and that of the emergence of definite form.

The final phase of form perception is the emergence of a clearly outlined and definite shape. This stage is represented cortically by a further development of

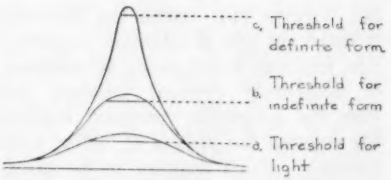

FIG. 1. The distribution of cortical excitation about the edge between stimulus figure and surround at the three stages of form perception.

those processes occurring during the second stage. The slope of the excitation gradient and the height of the distribution of activity will increase still more, and further peaking of the distribution will be revealed in greater sharpening of the activity peak. The sharpness of the edges of the figure concomitant upon still greater figure-ground contrast will lead to more nystagmatic oscillation, and the activity of the "on-off type" fibers will be increased. This stage is shown in Fig. 1*c*.

The part played by attention in the determination of the form threshold has been stressed by Marshall and Talbot:

> The gradients constructed from the image do not define activity until they encounter some ultimate *threshold*, which transects the peaks and gradients of activity. This would allow for subvisible neural action leaving traces in memory. . . . This threshold level, moreover, may be controllable within limits by the observer, and be the physiological effector of attention. . . . This local control of threshold would be "driven" by the effort to complete the pattern, initiated at a higher synaptic level (19, p. 141).
>
> Attention processes conceivably act by manipulating thresholds at some synaptic level. One can imagine a process in which the attention mechanisms seek out the peaks or gradients, literally trying to fit the various patterns, to determine which pattern can be seen with minimum effort (19, p. 150).

The importance of attention in manipulating threshold, and its significance in the total perceptual process, has been emphasized also by Hebb (10)

and Bartlett (1). Hebb has discussed in detail the role of attention in perception, referring it to a partly autonomous, or nonsensory cerebral activity; and from the subjective viewpoint Bartlett has stressed the process of "effort after meaning" which appears to be essentially the same notion as that introduced by Marshall and Talbot.

Application of the Statistical Hypothesis to Some Form-Perception Data

Emergence of form. Bitterman, Krauskopf, and Hochberg (2) used nine geometric forms; circle, square, diamond, equilateral triangle, cross, and L, X, T and H shapes, each with an area of .25 sq. in. In addition a diamond and an X shape equal in area to 1.00 sq. in. were used. Each observer was dark adapted for 10 minutes, and then shown the figures illuminated for .5 sec. every 2 sec. against a dark ground. At the indefinite form stage the square was called a circle, the triangle a circle, the cross a diamond, the X a square, the T a triangle with apex down, the L a semicircle or half-moon shape, and the H was sometimes reported as a butterfly shape. The circle was rarely perceived as anything but a circle. On methodological grounds, only a single threshold determination was made for each form. Helson and Fehrer (12) measured foveal thresholds for six figures of equal area; triangle, rectangle, square, semicircle and angle (L shape). In this experiment, determination of threshold in terms of intensity of illumination was made by the method of adjustment, so that time of exposure was not controlled. Essentially the same results as those obtained by Bitterman *et al.* are reported, although there are occasional differences as to which shapes were perceived at the threshold for indefinite form.

In order to interpret these data using the statistical theory, the case of the cross perceived as a diamond at the threshold for indefinite form will be taken as an example. It has been pointed out that, at the stage of indefinite form, the distribution of cortical excitation about an edge is at, or possibly just in excess of, that level required for the recognition of contour, although the sharpness of the activity peak is insufficient to attribute definite form qualities to it. Figure 1*b* shows the slightly flattened and unpeaked distribution in which the maximum effective value (i.e., width of distribution at the threshold level) would extend out into the angle formed by the intersecting arms of the cross, thus creating the subjective impression of a triangle rather than that of an open angle. The flatness, and hence the greater extent of the maximum excitation range, causes a "filling in" of the angle so that, taking the cross as a whole, the subjective appearance will be that of a diamond shape rather than that of a cross. Further, the "filling in" effect, resulting from the relatively wide extent of the maximum effective value of the excitation ridge, will give also the impression of blurredness, since the gradient of excitation represented in the slope of the curve falls away gently, failing thus to delineate subjectively areas of clearly different contrast. Therefore, the spaces between edges meeting at an angle will be filled in and, under the present conditions, this filled-in area will shade off gradually to give the impression of blurring. There will be, in other words, a gradation of brightness, from the brighter inner region of the angle formed by the two edges to the outer region, which will be darker than the former, and which will shade gradually into the dark surround.

With further increases in the brightness of the figure, the excitation distri-

bution about the cortically represented edges will grow peaked as vertical summation becomes more prominent, and the width of the maximum density of activity narrows. The effect of these changes in the nature of the cortical excitation ridge will be to lessen the blurring and the extent of filling in about the borders of the cross. This stage is represented in Fig. 1*c*.

The processes described serve to explain the indefinite form stage reports of X shape as a square, and the T shape as a triangle with apex down. Perception of the H shape as a butterfly (2) requires further qualification. In an earlier experiment (12) this shape was reported as a square at the threshold for indefinite form. Since there is no reason to doubt the accuracy of the subjective impressions reported by observers in the two experiments, it is very likely that the "butterfly" and "square" reports represent different stages in what is a continuous process of the emergence of definite form. The butterfly stage is somewhere intermediate between that of a square and definite form. Bitterman *et al.* (2) have been careful to point out that, in the experiment by Helson and Fehrer (12), repeated threshold determinations were made for each figure, so that observers became aware that only one of six forms could possibly emerge. Such a knowledge of the stimulus figure could easily lead to an overlooking of the butterfly impression, since observers would in their "effort after meaning" (1) perceive this as the H. Previous knowledge of the figure would lower the threshold for its definite form in the manner described above (10, 19). The same argument applies in the case of the indefinite form stage reports of the L shape as a triangle (12) and as a half-moon or semicircular shape (2).

Most experiments of this nature are unanimous in showing that the most common indefinite form stage report for the square is that of a circle, although this particular "wrong" response occurs in only about 10 per cent of the responses (12). This percentage of wrong responses is much less than that found, say, for an angle which is in the vicinity of 40 per cent. The blurring effect, resulting from the flattened or nonpeaked excitation distribution described above, would account for such responses. It is noteworthy that squares and rectangular figures have a consistently low threshold for definite form (2, 17) indicating, as would be expected from this hypothesis, that the external right angles of these figures do not for long remain obscured by blurring. Internal angles formed by indentations and protuberances in the outline of the figure would be very much more prone to filling in and blurring effects.

The processes described above to explain the perception of relatively simple geometric shapes probably occur also in relation to very much more complex figures. Before the threshold for definite form has been attained, complex silhouette pictures are erroneously perceived (5) so that the silhouette of a ship at sea is perceived as "a dog with a long tail," and that of yachts at sea as "islands with trees." As would be expected with more complex stimulus figures, the erroneous reports at the threshold for indefinite form tend to be less consistent between observers than is the case with simple geometric forms.

Area and area-perimeter ratio. It has been shown (2, 7, 17) that large figures have a lower threshold for definite form than smaller figures. It follows from the argument presented here that the larger the figure the less will be the filling-in effect resulting from the flattened excitation curve at the cortex. Presumably the width of the excitation ridge is the same about an edge, no matter what the size of the figure. With large and small figures of the same perimeter-to-

area ratio, figure details in the nature of indentations and protuberances would be less obscured, as a result of the flattened distribution, with a large figure than would be the case with a small figure of the same shape.

Threshold for definite form is affected also by the ratio of perimeter to area (2, 14, 17). The greater the perimeter of a figure in relation to area (i.e., the greater the P/A ratio) the greater will be the detail in the outline of the figure and thus the more complex the form. In terms of the present hypothesis, edge detail such as notches, indentations, serrations, and projections would be more subject to obscuring in the manner outlined for the open angle of a cross. This would result in a higher threshold for figures of greater P/A ratio than for figures of the same area but smaller P/A values. Although no data are available, the present hypothesis suggests that the greater the P/A ratio the greater will be the inconsistency of wrong responses at the threshold for indefinite form.

Important evidence relating to the effect of magnitude of detail on form threshold has been collected by Fry (7), who used fine and coarse saw-tooth edges between areas of different brightness. Contrast values for definite form were found for four patterns of the same over-all length, but in which the number of teeth varied from 2 to 16 and in which the visual angle subtended by one segment of the pattern varied from 83′ to 10.4′. With the finest pattern (16 teeth) two thresholds were found; one at which the saw-tooth contour was perceived as a straight line (indefinite form) and a second at which individual saw-teeth were visible (definite form). With the three coarser patterns only one threshold was observed, i.e., individual saw teeth were visible wherever the contour was visible at all. Since these experiments were carried out under normal lighting conditions, this latter result would be expected from the present hypothesis. That is to say, since the brightness of one edge relative to the other was increased from a level at which vertical summation would in all probability be active, the edges would not be blurred to the same extent had the eye been dark adapted first and contrast increased from a low initial brightness. Under these latter conditions a wavy edge probably would have preceded the appearance of the coarse saw-tooth border, since the emergence of definite form would be relatively slower. Fry obtained essentially the same results with fine and coarse wavy line patterns. It is interesting to note that these data have been interpreted (2) as clearly suggesting the operation of a diffusion process. The interpretation offered here serves equally well in dealing with these results, as well as remaining in much closer and realistic contact with neurological theory. Finally, Marshall and Day (18), using a grating test object constituted of bars subtending a visual angle of 12′, and serrated turret-fashion along one edge, have also observed the tendency to perceive the border as straight during the indefinite form stage. Further evidence in support of the statistical theory is provided by the above data relating to the perception of saw-tooth, wavy line, and serrated edges. The tendency to perceive these edges as straight at low contrast is a result of the blurring and obscuring of fine detail arising from an extended and flattened distribution of cortical excitation.

CONCLUSIONS

The arguments presented here are tentative, and in many respects speculative, yet they offer a consistent theory of form perception as an alternative to the diffusion hypothesis, which lacks neurophysiological foundation. The theory from which these arguments have been derived, although itself speculative

to a certain degree, is based firmly upon neurophysiological and anatomical facts. It also has the less important but nevertheless relevant commendation that it has proved fruitful in dealing with other aspects of visual form perception (21). For the purposes of clear and detailed exposition, this paper has been confined largely to the treatment of form perception at low levels of illumination, with increasing contrast between figure and ground. The theory can be extended, however, to treat further data of form perception closely related to the data dealt with here. For instance, there is now a considerable body of evidence (4, 6, 8, 15, 20, 25, 27) concerning the perception of form at varying distances from the fovea.

As the retinal image of the stimulus figure is located closer to the fovea, so form grows increasingly clear. In much the same manner as with foveal presentation at low illumination, the results from investigations employing the campimetric technique indicate that perception of the figure passes through a number of stages before it is perceived clearly. Thus Zigler *et al.* (27) have observed four stages: the figureless field, formless field, formlike figure, and clear figure. The last three of these are probably analogous to the three perceptual stages observed as the contrast between a foveally viewed figure and its surround is gradually increased. Similarly, during the stage preceding that of clear perception the same "wrong" responses are given frequently, with considerable consistency. Since vertical summation would be expected to occur to a greater degree at retinal loci close to the fovea, it follows that edges and contours would increase in clarity and definiteness as the figure is viewed at decreasing angles from the foveal line of regard. Without entering into a detailed analysis of peripheral form perception, the available evidence does indicate a close agreement between experimental findings and the present hypothesis.

REFERENCES

1. BARTLETT, F. C. An experimental study of some problems of perceiving and imaging. *Brit. J. Psychol.*, 1916, **8**, 222–266.
2. BITTERMAN, M. E., KRAUSKOPF, J., & HOCHBERG, J. E. Threshold for visual form: a diffusion model. *Amer. J. Psychol.*, 1954, **67**, 205–219.
3. BYRAM, G. M. Physical and photochemical basis of visual resolving power. Part II. Visual acuity and the photochemistry of the retina. *J. Opt. Soc. Amer.*, 1944, **34**, 718–738.
4. COLLIER, R. M. An experimental study of form perception in indirect vision. *J. comp. Psychol.*, 1931, **11**, 281–289.
5. CRAIK, K. J. W., & VERNON, M. D. Perception during dark adaptation. *Brit. J. Psychol.*, 1942, **32**, 206–230.
6. DRURY, M. B. Progressive changes in non-foveal perception of line patterns. *Amer. J. Psychol.*, 1933, **45**, 628–646.
7. FRY, G. A. The relation of the configuration of a brightness contrast border to its visibility. *J. Opt. Soc. Amer.*, 1947, **37**, 166–175.
8. GEISSLER, L. R. Form perception in indirect vision. *Psychol. Bull.*, 1926, **23**, 135–136.
9. HARTLINE, H. K. The responses of single optic nerve fibres of the vertebrate eye to illumination of the retina. *Amer. J. Physiol.*, 1938, **121**, 400–415.
10. HEBB, D. O. *The organization of behavior.* New York: Wiley, 1949.
11. HELSON, H. Dr. Wilcox on the role of form in perception. *Amer. J. Psychol.*, 1933, **45**, 171–173.
12. HELSON, H., & FEHRER, ELIZABETH V. The role of form in perception. *Amer. J. Psychol.*, 1932, **44**, 79–102.
13. HILL, A. V. Excitation and accommodation in nerve. *Proc. roy. Soc., Ser. B*, 1936, **119**, 305–355.
14. HOCHBERG, J. E., GLEITMAN, H., & MACBRIDE, P. D. Visual thresholds as a function of simplicity of form. *Amer. J. Psychol.*, 1948, **3**, 341–342.
15. KLEITMAN, N., & BLIER, Z. A. Color and form discrimination in the periphery of the retina. *Amer. J. Physiol.*, 1928, **85**, 178–190.
16. KÖHLER, W., & WALLACH, H. Figural after-effects. *Proc. Amer. Philos. Soc.*, 1944, **88**, 269–357.

17. KRAUSKOPF, J., DURYEA, R. A., & BITTERMAN, M. E. Threshold for visual form: further experiments. *Amer. J. Psychol.*, 1954, **67**, 427–440.
18. MARSHALL, A. J., & DAY, R. H. The resolution of grating test objects during the course of dark adaptation. *Austral. J. Psychol.*, 1951, **3**, 1–21.
19. MARSHALL, W. H., & TALBOT, S. A. Recent evidence for neural mechanisms in vision leading to a general theory of sensory acuity. In H. Klüver (Ed.), *Visual mechanisms*. Lancaster: Jaques Cattell, 1942. **Pp. 117–164.**
20. MUNN, N. L., & GIEL, G. A. A note on peripheral form discrimination under dark adaptation. *J. gen. Psychol.*, 1931, **5**, 78–87.
21. OSGOOD, C. E., & HEYER, A. W. A new interpretation of figural after-effects. *Psychol. Rev.*, 1952, **59**, 98–118.
22. PARSONS, J. H. *An introduction to the theory of perception.* Cambridge: Cambridge Univer. Press, 1927.
23. POLYAK, S. L. *The retina.* Chicago: Chicago Univer. Press, 1941.
24. SMITH, K. The statistical theory of the figural after-effect. *Psychol. Rev.*, 1952, **59**, 401–402.
25. WHITMER, C. A. Peripheral form discrimination under dark adaptation. *J. gen. Psychol.*, 1933, **9**, 405–419.
26. WILCOX, W. W. Helson and Fehrer on "The role of form in perception." *Amer. J. Psychol.*, 1932, **44**, 578–580.
27. ZIGLER, M. J., COOK, B., MILLER, D., & WEMPLE, L. The perception of form in peripheral vision. *Amer. J. Psychol.*, 1930, **42**, 246–259.

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